

SCIENCE.

FRIDAY, OCTOBER 5, 1883.

NATIONAL TRAITS IN SCIENCE.

THERE are at present three principal currents of scientific work, — German, English, and French. The scientific writings of each nationality are characteristic, and, taken as a whole, offer in each case distinctive qualities. German influence is now predominant over the scientific world, as French influence was uppermost during the earlier part of this century; but the sway of Germany over western thought is far more potent and wide-spread than was ever that of France. As students once gathered in Paris, so they now flock to Germany; and thence back to their own lands they carry the notions of German science, and labor to extend, imitate, and rival them. Thus German ideas have been spread abroad, and established in foreign countries. This has set a common standard for scientific work, which is accepted in most European countries. German influence is evident by its effects in Switzerland, Russia, Italy, Poland, Belgium, England, and America, and in degrees indicated by the order given: in France, Spain, and Portugal, it is hardly noticeable. Holland and the Scandinavian countries have for many years achieved so much and so excellent work, that their scientific development may be said to have accompanied rather than to have followed that of Germany.

German science has unquestionably distinctive qualities. Its pursuit is a special and honored calling, attractive to the highest talent: its productions have the stamp of professional work. The German scientific man is first and principally an investigator: he is obliged to be so, otherwise he loses in the race. He wins his position in the hierarchy of learning by the original researches he carries out. To succeed under these circumstances, a man must discover something which is a real addition to

knowledge; and to do this, he must be thoroughly familiar with all that has been previously accomplished in his field. Moreover, to advance beyond his peers, the investigator must utilize every possible extraneous advantage; more especially must he have a mastery over the methods to be employed, and be familiar with all novelties and refinements therein. It cannot be gainsaid that these requirements are more fully answered in Germany than anywhere else. It is certain, that, excepting of course a small minority, German scientific publications always contain something really new, and unknown before: each article is a scientific progress, which, however slight, still brings an actual increment to our store of information. Another result of this professional thoroughness is equally striking and characteristic. Being fully posted as to the status of his department, the German often displays a singularly just and keen appreciation of what problems are for the moment best worth studying, as being open for solution, and leading to something farther, or else filling a gap left. He is thus enabled to render his work efficient. It is sad to think how much scientific work is wasted because the labor is not wisely directed.

In German scientific writings the excellence of the matter usually contrasts vividly with the defective style and presentation. Indeed, the Germans, despite the superiority of their modern literature, are awkward writers, and too often slovenly in literary composition. Conciseness and clearness are good qualities, which may assuredly be attained by the expenditure of thought and pains; but these the German investigator seems unwilling, in many cases, to bestow upon his pen-work, but follows the easier plan of great diffuseness. Besides this, another defect is not uncommon, — the ill-considered arrangement of the matter. This occurs in all degrees, from a well-nigh incredible confusion, to be sometimes found even in elaborate and important essays, to a slightly

illogical order. In this regard, a curious and not infrequent variety of this fault deserves mention. According to the headings of the chapters or sections, the division of topics is perfect; but under each head the matters are tumbled together as if a clerk was contented to stuff his papers in anyhow, if only he crammed them into the right pigeon-hole.

Speaking broadly, the German mind lacks conspicuously the habits of clearness and order. There have been celebrated exceptions, but they were individual. The nation regards itself as having a decidedly philosophical bent, meaning a facility at taking broad and profound views of the known. We venture to contradict this opinion, doing it advisedly. Their profundity is mysticism, their breadth vagueness, yet a good philosopher must think clearly. It is a remarkable but little heeded fact, that Germany has not contributed her share to the generalizations of science: she has produced no Linné, Darwin, Lyell, Lavoisier, or Descartes, each of whom bequeathed to posterity a new realm of knowledge, although she has given to the world grand results by the accumulated achievements of her investigators. The German's imperfect sense of humor is another obstacle which besets him on every path. He is cut off from the perception of some absurdity, like that of Kant's *neumenon*, for instance. One cannot explain this to him: it were easier to explain a shadow to the sun, who always sees the lighted side. To state the whole epigrammatically, German science is the professional investigation of detail, slowly attaining generalizations.

English science is the opposite of this,—amateurish rather than professional. Some might call it insular, yet we should hardly join them in so doing. In fact, the professional investigator has hardly been a recognized character in the English social organization: until recently he was barely acknowledged, even by the universities, which sought instructors who knew and could teach, who might investigate and discover in a subsidiary, and, as it were, unofficial way. A large number of English

scientific men were disconnected from the universities and colleges after their own student years, and were half or wholly amateurs; and their writings show the effects of this separation, not always, to be sure, but in many cases with painful evidence, by a lack of thoroughness, an imperfect acquaintance with other investigations, and a failure to grasp the essential part of the problem: in brief, such writings appear behindhand and superficial. Yet amid these poorer productions are to be found a right goodly number of the best scientific articles we possess in any language. Of late years the proportion of the good has steadily increased, and investigation is now more correctly appreciated than ever before. Indeed, there is no more encouraging event in the recent progress of science than the sudden elevation of the standard of original research in England. The English are trained writers: their scientific articles excel the German in literary merit, being seldom slovenly either in arrangement or style, and rarely wearisome from sheer diffuseness. Very noteworthy is the fertility in generalizations of the English: this is with them the outcome of individual endowments, a single master attaining a broad conclusion,—a process of individual effort quite unlike the German democratic method of generalizing by the accumulations of many. Is it too much to say that the English and Scotch are the Greeks of modern philosophy?

French science is decidedly provincial: it is apart, having only an imperfect, uncertain acquaintance with the great world outside, and its international interests of original research. The French have lagged far behind the great movements of recent years. Consider only how backward they have been in the comprehension and acceptance of the Darwinian theory; and remember, too, that it were wiser to take out the mainspring from a watch than to eliminate evolution from biology. French scientific articles are well written, the matter is admirably classified, it is all very clear. The keen, artistic sense of the nation displays itself here; but it also deludes them into presenting a rounded survey of a greater field than is demanded by

the actual discoveries they report. To satisfy this yearning for artistic completeness, elaborate and tedious disquisitions, and hackneyed principles, and facts long known, are interpolated; and even worse may be, when the imagination helps to create the completeness. Most scientific men harbor a little distrust of French work. This sentiment is further fostered by the almost systematic neglect of German research on the part of the French. Such a frank exhibition of rancor makes one suspect the impartiality of the French in science generally: indeed, we believe that science has never been so depressed in France as at present. Italy is above her; but Italy, with all her innate ability, is striving to learn from Germany, and has already risen high, and will rise higher. We trust and believe that the present phase of French science which abounds in inefficient work will soon end, and the people terminate their present voluntary isolation. The French stay at home: they used to travel abroad much. Let us hope that they will soon resume their ancient habit, and, above all, that they will re-establish mental intercourse with foreigners. There are *savants* in France who are esteemed throughout the scientific world: may their number rapidly increase!

America's contributions to pure science are by no means very extensive, or often very important: compared with the great volume of German production, they seem almost insignificant. We have never duly fostered research, for we have bestowed upon it neither the proper esteem nor office. There are, we suppose, at least six thousand 'professors' in the United States: are one hundred and fifty of them active investigators? The time seems remote when every American professor will be expected to be also an investigator; but among us is a little band of men who have before them the model of Germany, and who are working earnestly for the intellectual elevation of their country. Their first object is necessarily to render research more important in public estimation, and so to smooth the way for a corps of professional investigators. Every thoughtful person must wish success to the attempt.

CLIMATE IN THE CURE OF CONSUMPTION.¹—II.

Humidity.

THERE is a unanimity of opinion amongst authorities in regard to the relation of moisture to the production of phthisis. The seventh annual report of the registrar-general of Scotland showed that the death-rate from phthisis diminished in proportion to the dryness of the location. Dr. H. I. Bowditch of Boston has shown that phthisis is prevalent in damp soils in the United States. "It is also common in Holland, and other countries liable to damp fogs and an atmosphere saturated with moisture" (Reynold's System of medicine, iii. 548). Ruehle, in Ziemssen, says, "It appears that moist air favors consumption." Dr. Austin Flint says, "It may be stated that the prevalence of the disease is less in climates either uniformly warm and dry or uniformly cold and dry." And Dr. C. T. Williams writes, "As to the desirability of moist climates for consumptive patients, the evidence is decidedly against their use in the treatment of ordinary chronic phthisis."

If we attempt to explain why it is that phthisis is more prevalent in moist climates than in dry, we might assign as a cause the prevalence of germs, or the impurity of the air, containing the effluvia of decay, or perhaps the greater susceptibility of the system to cold in moist climates; or it may be that the air, being so near saturation, cannot take up the requisite amount of the aqueous vapor exhaled from the lungs. *Causa latet vis est nota* may adequately express the state of our knowledge in regard to this point. A moist climate is acknowledged to be a breeder of phthisis; and, *au contraire*, a dry climate is known to afford a certain exemption from the disease. This is shown by the fact that the disease is rare in Iceland, in the island of Morstrand, on the steppes of Kirghis, and in the interior of Egypt; in all of which places the element of elevation is wanting. It may, then, be conceded, that dryness of the air is an important element in the prophylaxis and cure of phthisis.

The method of determining the humidity of the air is that introduced by Regnault, known as the wet- and dry-bulb test. It can easily be seen that the results obtained will depend on the exposure of the thermometers, and on the accuracy of the readings. Moreover, the amount of moisture that the air is capable of

¹ Concluded from No. 34.

holding varies with the atmospheric pressure and temperature.

While it seems to us that a table showing the relative humidity, i.e., the percentage of saturation of the air, would be sufficiently accurate as a basis of comparison, yet, as it might be objected that such a table would be subject to error, we have appended another table, giving the absolute moisture, or the number of grains of vapor to the cubic foot of air. This second table we have computed from Glaisher's tables.

Consulting these tables (table I., columns iii. and iv.), it is seen that Denver and Santa Fé afford a very low relative and absolute amount of atmospheric moisture, — a relative amount, which, as between Denver and Jacksonville, is as 1 to 3, and, as between Denver and Los Angeles, is as 1 to 2.

This proves, that, on the eastern slopes of the Rocky Mountains, we have, in addition to the favorable element of elevation, a second, that of dry air, as an element of climatic influence in the cure of phthisis.

Precipitation.

Closely related to the foregoing, is a consideration of the mean annual precipitation, or the mean annual amount (in inches) of rain and melted snow. Its bearing on our subject is apparent in several ways.

1. Of the precipitation, a certain part is lost by evaporation, and tends to increase the humidity of the air. This amount will depend upon the amount of moisture in the air, or its degree of saturation, and also upon the amount of the precipitation left upon the surface of the ground to be evaporated. It is evident that the greater the porosity of the soil, the greater will be its absorptive power, and the less the evaporation from it. Such a porous soil is found on the eastern slopes of the Rocky Mountains. Loose, sandy, and gravelly, it eagerly drinks up all the rainfall; and such a thing as mud is rarely seen.

2. It is well known that pulmonary troubles are most prevalent during 'thaws,' in those places where the snow lies upon the ground in winter. Now, in the district of the Rocky Mountains under consideration, there is, in the first place, only a slight amount of snowfall, so that sleighing is exceptional, and, in addition, the warm sun soon melts the snow, and the thirsty, porous soil drinks it up; so that the annual 'spring thaw' of our Eastern States is a *res incognita* in this country. The writer remembers very distinctly several snowfalls of

fourteen to twenty-two inches on a level, of which there was not a vestige left in ten days; and during that time the air was not chill and raw, and there was but little slush.

3. Further than this, the amount of the precipitation has a bearing upon our subject, as indicating approximately the ability of the invalid to lead an out-of-door life. We shall defer our discussion of this point to a later part of this paper.

Turning, now, to the tables, we see (table I., column v.) that in Denver the mean annual precipitation for a period of ten years is only 14.77 inches in rain and melted snow, — an amount which is only one-fourth of that at Jacksonville, and which, with Santa Fé, gives the smallest showing in our range.

We can therefore add this element of climate to the other two of elevation and dry air as a point in favor of the Rocky Mountains in the cure of phthisis.

Temperature.

The writer in Reynold's System (*op. cit.*) says of this matter of the relation of the temperature of climate to the cure of phthisis, "It was formerly supposed that warm climates were beneficial for consumptive patients. . . . But it will be invariably observed that unaccustomed warmth is injurious. . . . What is really required, is a cool, temperate climate, free from great alterations of temperature." Dr. Austin Flint (*op. cit.*) calls attention to the fact, that "the disease is oftener developed during the spring months and the hot months of summer," when either there is a great deal of moisture in the air, or the debilitating effects of heat are present as factors. On the other hand, Ruehle says that the temperature has "nothing to do with the prevalence of consumption."

It is known that the effect of heat is to raise the body temperature, to lessen the number of respirations, to quicken the pulse, to lessen the digestive powers and the appetite, to diminish the excretion of urea because of the diminishing of the ingesta, and to depress the nervous system, especially if the heat be accompanied with excessive moisture. It seems, then, that it can be stated as a fair inference from the foregoing, that a dry, temperate climate is to be sought by the phthisical invalid. The Rocky Mountains furnish a dry climate. The table (table I., column vi.) shows that the mean temperature is nearly a mean between the extremes in our range. The question will, however, be presented in a better form farther on.

Winds.

The points of importance in regard to the winds are their velocity and direction. It is well known that they are regulated somewhat by changes in atmospheric pressure and temperature.

Velocity. — It is known that a cold wind abstracts body-heat, and in proportion to its velocity. By consulting our tables (table II., part ii.) it will be seen that the mean daily velocity of the winds at Denver is less than it is in the Eastern States; and that as a consequence, while the mean temperature is nearly the same, the chilling effect will be much less. On the other hand, as it has a considerably greater velocity, and as there are fewer calms than at either Augusta or Los Angeles, it has a proportionately greater purifying power in bringing fresh ozone, and in blowing away the products of decomposition.

Direction. — Of more importance than the velocity, is the direction of the winds. The favorable and unfavorable directions vary for different places, according to their geographical location. The east and north winds are known to be the trying ones along the Atlantic coast; and our table shows that the north-east wind is the prevailing one at both Augusta and Jacksonville. The west wind, blowing from the Pacific Ocean, and bringing fogs, is the trying one on the California shore; and the table shows that this is the prevailing one at Los Angeles. The south wind is the salubrious one for the eastern slope of the Rocky

Mountains, in Colorado; and our table shows that this is the wind that blows there most frequently.

We can therefore add this element to the others, — of elevation, dryness of air, small amount of precipitation, and mean temperature, — as favorable to the Rocky Mountains as a place for phthysical patients to resort.

Clear, fair, and cloudy days.

We now come to the consideration of our last general point, that is, to an investigation of the number of clear, fair, and cloudy days; or, in other words, to a consideration of the amount of sunshine.

As to the direct effect upon health produced by light and sunshine, we are still in ignorance. Whether the blood is made to course more rapidly, and the nerves transmit impulses more readily, under the influence of the solar ray, is not known. It is well known that the actinic rays have a powerful chemical effect upon vegetation; but whether or not they have a like influence upon the human economy is unknown.

Without attempting to refine, there are certain broad and positive effects in the cure of phthisis attributable to sunshine. The experience of the profession is fittingly expressed by the words of Dr. Austin Flint: "I would rank exercise and out-of-door life far above any known remedies for the cure of the disease."

TABLE II. — WINDS.

STATION.	DIRECTION.										VELOCITY.	PREVAILING WIND.
	MEAN FOR THREE YEARS.											
	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.			
											Mean daily.	Mean 5 years.
Augusta, Ga.	57	154	68	118	86	109	94	120	304		79 miles.	N.E.
Jacksonville, Fla.	77	282	120	116	102	180	83	76	60		159 "	N.E.
Boston, Mass.	70	59	88	72	85	155	286	186	60		238 "	W.
Newport, R.I.	113	80	51	73	112	299	183	164	28		236 "	S.W.
New York, N.Y.	76	136	54	98	83	175	188	290	14		207 "	N.W.
Philadelphia, Penn.	123	157	92	40	102	162	179	208	9		256 "	S.W.
Chicago, Ill.	147	122	85	99	142	239	151	114	30		203 "	S.W.
St. Paul, Minn.	95	37	91	223	72	122	129	223	70		211 "	S.E.
Denver, Col.	150	95	78	150	307	67	76	155	12		145 "	S.
Santa Fé, N. Mex.	139	109	157	100	78	142	59	119	201		152 "	E.
Salt Lake, Utah	81	64	56	252	39	33	49	269	249		135 "	N.W.
Los Angeles, Cal.	104	155	80	58	55	208	274	53	142		103 "	W.

In the table which we present (table III.), and which is a mean of daily observations for a period of five years, a cloudy day is one in which the heavens are from seven-tenths to entirely obscured by clouds; a fair day is one in which the heavens are from four to seven-tenths clouded; all else are classed as clear days. From this it will be seen, that for our purposes clear and fair days may be classed as one, and may be put into juxtaposition with the cloudy days. Consulting these tables, it will be seen, that in Denver the mean number of cloudy days in a year is only one-half of what it is in either Augusta or Jacksonville, that it is less than a half of what it is in St. Paul, and that it is slightly less than what it is in Los Angeles.

TABLE III.

STATION.	MEAN FOR 5 YEARS.		
	Clear.	Fair.	Cloudy.
Augusta, Ga.	123	150	92
Jacksonville, Fla.	126	152	87
Boston, Mass.	105	145	115
Newport, R.I.	108	140	111
New York, N.Y.	101	155	109
Philadelphia, Penn.	114	139	112
Chicago, Ill.	104	154	107
St. Paul, Minn.	103	158	104
Denver, Col.	177	142	46
Santa Fé, N. Mex.	174	148	41
Salt Lake, Utah	141	131	93
Los Angeles, Cal.	164	148	51

To put this fact in another way, it is seen, that in Denver there is only about one-eighth of the entire year when an invalid would be kept in the house on account of the weather; in Jacksonville and Augusta he would be confined to the house, for the same reason, one-quarter of the year; in St. Paul he would be kept in-doors between a third and a quarter of the time; while in Boston he would have to be housed a good third of the time.

Admitting, then, the force of Dr. Flint's statement, our tables show that there is no place in this whole country, where it is possible for the invalid to enjoy so much fresh air and sunshine, as in the Rocky Mountains. For three hundred and twenty days out of every three hundred and sixty-five it is possible to roam at large, and to breathe in health.

We feel, that, so far, our tables have shown that the Rocky Mountains furnish climatic conditions of elevation, humidity, precipitation,

temperature, winds, and sunshine, which recommend them as a resort for phthical invalids superior to any thing to be found in this country.

Observations by seasons.

Having arrived at these general conclusions, the writer wishes to call attention very briefly to their accuracy and importance as applied to the different seasons of the year. He wishes to lay stress upon the evidence which goes to show that Colorado and New Mexico furnish favorable resorts for phthical invalids during the winter and spring,—the very seasons that are most trying in the east, the seasons that they are obliged to avoid, and to seek new abodes at the resorts. The elements of elevation and barometric pressure will remain nearly constant the year round. But how is it in regard to the *humidity* of the air in Colorado during the winter and spring? The writer has selected at random, and without reference to whether the showings would be favorable or unfavorable for a given place, the year 1880 as his basis for comparison. By referring to table IV., part i., it will be seen, that both the relative and absolute humidity for Denver during the winter and spring is absolutely, and by comparison, very small; that, as compared with Augusta and Jacksonville, it makes a wonderful showing in these respects; and that the ratio of the absolute humidity as between Denver and Los Angeles is as 1 to 3 for these seasons.

When we turn to our tables (table IV., part ii.), we learn that the amount of *precipitation* at Denver for these seasons was almost *nil*; that the mean monthly precipitation at Denver for the given time was only a small fraction of an inch in rain and melted snow. Carry out, now, the comparison between Denver and Augusta, Jacksonville and Los Angeles, and see the tremendous difference in this particular between these places,—a showing immensely in favor of Denver. It will be seen that our general conclusions are very much strengthened by this particular application, and that we have brought strong additional evidence in favor of Colorado as a resort for persons affected with phthisis pulmonalis.

When we turn to our tables to learn in regard to the *winds* at these places for the given seasons, we see that the conclusions previously reached in regard to Denver, in this particular, still hold true (table V.).

Temperature.—We come now to our last observation, and to a brief discussion of what some may consider the weak point in regard

TABLE IV.

STATION.	1880. RELATIVE AND ABSOLUTE HUMIDITY.								1880. PRECIPITATION.			
	Spring.		Summer.		Autumn.		Winter.		Spring.	Summer.	Autumn.	Winter.
	R. H. ¹	G. V. ²	R. H.	G. V.	R. H.	G. V.	R. H.	G. V.				
Augusta, Ga.	68	4.7	64	7.2	74	4.9	72	3.2	5.0	4.2	2.8	3.9
Jacksonville, Fla.	66	5.5	69	8.0	75	5.6	70	3.0	3.5	5.9	9.5	3.5
Boston, Mass.	61	2.5	70	5.4	67	3.0	69	1.5	2.6	3.5	2.6	3.8
Newport, R.I.	71	2.7	76	5.9	75	3.5	76	1.9	3.9	5.0	3.4	3.6
New York, N.Y.	65	2.9	79	5.5	70	3.8	65	2.0	2.5	4.2	2.5	2.8
Philadelphia, Penn.	61	2.7	66	6.1	68	3.0	72	2.1	2.1	4.8	1.5	2.6
Chicago, Ill.	63	2.9	70	5.9	66	2.3	69	1.8	4.1	3.7	2.1	2.4
St. Paul, Minn.	62	2.9	69	5.1	66	2.9	71	-	2.1	4.0	2.3	1.5
Denver, Col.	37	1.1	43	3.3	55	2.3	51	0.9	0.5	1.3	1.0	0.2
Santa Fé, N. Mex.	35	1.2	36	2.6	50	2.5	50	-	0.2	1.7	0.7	0.6
Salt Lake, Utah	41	1.5	25	1.5	33	1.6	50	1.3	1.6	0.3	0.7	1.1
Los Angeles, Cal.	74	3.6	73	4.8	63	3.8	68	2.9	2.2	0.6	0.3	3.8

¹ Relative humidity.² Grains vapor.

to Colorado as a resort for invalids. It has been seen that most authorities favor a cold climate; but they add the proviso that it should be free from change. By consulting table VI., part ii., it will be seen that the mean monthly range of temperature is larger for Denver than for almost any other point in our scale. It will be seen, further, that the minima (table VI., part i.) of temperature are very

nearly the lowest in the scale, — not so low, to be sure, as the minima at St. Paul, but decidedly lower than at Augusta, Jacksonville, and Los Angeles. This state of affairs demands an explanation.

We have seen that the air of Colorado is both dry and rare, — two conditions that favor rapid radiation. We have seen, further, that the soil is of a porous, sandy nature, — a kind

TABLE V.—WINDS, 1880.

STATION.	DIRECTION.				VELOCITY, MEAN DAILY.			
	Spring.	Summer.	Autumn.	Winter.	Spring.	Summer.	Autumn.	Winter.
Augusta, Ga.	S.E.	S.E.	N.E.	N.W.	102	77	67	101
Jacksonville, Fla.	N.E.	S.W.	N.E.	N.E.	226	162	157	189
Boston, Mass.	S.W.	S.W.	W.	N.W.	257	184	223	281
Newport, R.I.	S.W.	S.W.	S.W.	S.W.	243	168	232	266
New York, N.Y.	N.W.	S.W.	N.W.	N.W.	247	177	192	222
Philadelphia, Penn.	S.W.	S.W.	N.W.	S.W.	294	218	228	178
Chicago, Ill.	S.W.	S.W.	S.W.	S.W.	182	183	217	193
St. Paul, Minn.	S.E.	S.E.	N.W.	S.E.	210	180	189	193
Denver, Col.	S.	S.	S.	S.	156	143	115	127
Santa Fé, N. Mex.	S.W.	N.E.	N.E.	N.W.	167	134	140	148
Salt Lake, Utah	S.E.	N.W.	S.E.	S.E.	122	130	102	103
Los Angeles, Cal.	S.W.	W.	W.	N.E.	187	131	78	112

TABLE VI. — TEMPERATURE, 1880.

STATION.	SPRING.		SUMMER.		AUTUMN.		WINTER.		SPRING.		SUMMER.		AUTUMN.		WINTER.	
	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Range.	Mean.	Range.	Mean.	Range.	Mean.	Range.	Mean.
Augusta, Ga. . . .	90	35	99	60	91	28	81	7	47	66	32	81	45	63	54	51
Jacksonville, Fla. . .	95	42	100	69	91	39	78	19	43	71	29	82	39	69	45	59
Boston, Mass. . . .	92	12	101	47	93	3	59	— 5	58	47	48	69	52	51	52	33
Newport, R.I. . . .	84	14	88	49	87	15	61	1	44	47	33	69	42	53	48	36
New York, N.Y. . . .	94	16	92	48	91	14	65	— 6	54	51	36	72	45	53	52	36
Philadelphia, Penn. . .	96	20	95	51	91	10	67	— 5	56	54	37	75	49	55	53	37
Chicago, Ill. . . .	85	19	95	52	85	1	63	—15	47	51	42	72	53	48	57	31
St. Paul, Minn. . . .	91	— 7	98	47	90	—18	59	—27	59	46	43	69	62	42	67	21
Denver, Col. . . .	89	—10	96	39	89	—13	59	—11	66	48	48	69	60	44	72	29
Santa Fé, N. Mex. . . .	80	0	88	33	80	—11	56	— 3	67	47	45	66	59	48	61	27
Salt Lake, Utah . . .	78	5	95	40	88	3	49	2	51	45	51	68	51	49	44	31
Los Angeles, Cal. . .	97	36	87	50	91	35	80	30	45	56	38	64	47	61	46	54

that will easily absorb heat, and as easily give it off. Furthermore, there is but little verdure or shade, — another condition, too, which will favor both absorption and radiation of heat. In consequence of these conditions, the soil and air are, on the one hand, rapidly heated in the morning, and they are equally rapidly cooled at night. The nights are always cool in Colorado, — a condition that renders the summer months enjoyable and invigorating. But the question, after all, is, whether this diurnal change of temperature is injurious to Colorado as a resort for invalids. We claim that it is not, and for this reason: it makes but little difference to the invalid how cold the nights are, for at that time he should be indoors, where he can regulate the temperature; but it is of importance that it should be warm at mid-day, so that he can take his exercise regularly and comfortably. We have seen, that so far as conditions of sunshine, humidity, and rain and snow fall are concerned, the invalid can lead an out-of-door life a greater percentage of the time in Colorado

than anywhere else in this country; and we claim that he will never find these factors counterbalanced by the element of temperature. An experience of several years warrants the writer in asserting that an invalid can, with perfect comfort and safety, spend several hours in the saddle nearly every day of the three hundred and sixty-five. One has but to read 'H. H.'s' writings to learn how attractive out-of-door life is in Colorado, even in mid-winter; and we can positively assert that we have known of picnics being held day after day, in the open air, in the very heart of the winter, and that there are days and weeks in mid-winter when one can sit with doors and windows open.

As proof of what we say, we append the mid-day temperature at Denver for each month of the year for three years.

In conclusion, the writer would state, that while his personal experience in regard to a desirable climate for the cure of phthisis has been such as to convince him of the great superiority of the climate of the Rocky Moun-

TABLE VII. — DENVER, MEAN MONTHLY TEMPERATURE, 1 P.M.

YEAR.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1880.	45°.7	37°.6	44°.3	56°.7	67°.2	78°.5	79°.9	79°.6	71°.3	56°.8	30°.3	35°.8
1881.	33.5	36.4	46.1	62.2	69	82.5	84.6	81.3	70	59.7	42.8	47.6
1882.	37	46.2	52.3	55.5	59.3	73	81.6	81.4	73.9	59.4	46.5	43.2

tains over any other in this country, yet in this article he has tried to put aside any personal bias, and he desires to carry conviction only in so far as he has been able to adduce facts, and to interpret them rationally and logically. He would state, further, that, if the reader should take exception to any interpretation given to the facts, the tables still stand as the best and most reliable data of these facts attainable, and they are not to be controverted.

SAML. AUG. FISK, M.D.

Denver, Col.

REARING OYSTERS FROM ARTIFICIALLY FERTILIZED EGGS AT STOCKTON, MD.

IN order to test the feasibility of such an undertaking on a considerable scale, a pond three and a half feet deep was excavated on the premises of Messrs. George V. Shepard and H. H. Pierce, not far from Stockton, near the shore of Chincoteague Bay, and connected with the latter by a trench ten feet long, two feet wide, and three and a half feet deep. Before the water was let into the trench connecting the pond with the bay, a wooden diaphragm—made in the form of a box three feet deep, and two and a half feet wide, and two inches thick, and lined on the inside with gunny-cloth; the sides of the box being perforated with numerous auger-holes, and filled with clean sharp sand, so as to form a filter—was placed in the trench vertically and transversely, and so secured that no water, except such as had first filtered through the diaphragm, could gain access to the pond. In this way the natural fry from the bay was effectually excluded from our artificial enclosure from the very start, so that the results of our experiment might not be vitiated. In the construction of this simple apparatus we depended entirely upon the rise and fall of the tide to partially renew the water in the pond in the intervals between the tides. The tidal rise and fall of the water in the enclosure was from four to six inches, or from six to eight inches less than the rise and fall of the tide in the open bay. Into the enclosure, covering about fifty square yards, artificially fertilized spawn was poured at odd dates, from July 7 to the first week of August.

The spawn was taken from the adults much in the same way as from fishes; the right valve of the adult animals being removed, and the ducts of the reproductive organs stroked with a pipette to force out the eggs and milt from the females and males. The products mixed together in water were then allowed to stand

in pails for a few hours, until the eggs had developed as far as the swimming stage. The spawn so prepared was then distributed over different parts of the pond, and left to take care of itself.

The collectors used were simply oyster-shells strung upon galvanized wire; strings of shells being suspended to stakes driven into the bottom of the pond at intervals corresponding to the dates when fresh lots of spawn were introduced, each stake being marked with the date when it was put in place. The suspended oyster-shells were introduced so as to afford the young fry clean surfaces to which it could attach itself. On the 22d of August Mr. Pierce found that some of the shells hanging to the stakes had spat attached, ranging from one-fourth to three-fourths of an inch in diameter, and which had undoubtedly been derived from some of the brood placed in the pond by us. Some specimens of these young oysters are now in my possession, attached to the perforated oyster-shells used as collectors.

To our great surprise, we found that the water in the pond maintained about the same specific gravity as that in the bay, or 1.0175 to 1.018, and that the temperature of the water was also the same as in the open bay. The microscopic vegetable food upon which the oyster feeds was found to multiply rapidly in the enclosure, inasmuch as it was confined by the gate or diaphragm, so that it could not escape. The water in the pond was also found to remain sweet, and free from putrefactive odors. It will accordingly be seen, that all the conditions of success had been established, as was fully proved by the result.

While it is too soon to affirm that artificial breeding may be profitably available on an extensive scale in practical oyster-culture, our experiment has demonstrated a number of very important facts. These are: 1. Oyster-spat may be reared from artificially fertilized eggs; 2. The spat will grow just as fast in such enclosures as in the open water; 3. Food is rapidly generated in such enclosures; 4. The density of the water in the ponds is not materially affected by rains, or leaching from the banks; 5. Ponds are readily excavated in salt-marsh lands, and can in all probability be used for fattening and growing *Ostrea virginica* for market just as successfully as *Ostrea edulis* and *angulata* are grown by a similar method on the coasts of France. Pond-culture, where there are salt-marshes adjoining arms of the sea the waters of which have a density below 1.020, can doubtless be carried on profitably in connection with the intelligent use of simple,

cheap collecting-apparatus placed in both open and confined waters to catch a 'set' of spat, which can then be transferred to ponds or open beds. The methods of spawn-taking and pond-culture introduced by the writer are inexpensive and very simple, and can be understood and conducted by any person of ordinary intelligence, and are fully described in papers already published, or in course of publication, by the U. S. fish-commission, under the auspices of which he has been enabled to carry out his investigations. The experimental difficulties have been overcome. It remains for practical men to avail themselves of whatever of value has been determined by these experiments. There are thousands of acres of salt-marsh land along the eastern coast of the United States, which, with proper preparation, might be made to yield a living to a large number of persons, and which is now not productive of any thing except mosquitoes and malaria.

Pond-culture has one other decided advantage over culture in the open water; namely, that it is possible to effectually exclude from the artificial enclosures certain enemies of the oyster, such as whelks and star-fishes.

J. A. RYDER.

THE EXPLOSION OF THE RIVERDALE.

THE boiler of the steamer Riverdale exploded on the 28th of August, a few minutes after the boat had left her wharf at New York, and started for her destination on the Hudson River above the city. Several lives were lost, and the boat itself was sunk in sixty feet of water. The boiler was raised, and placed upon the wharf near the Delamater iron-works; and the boat, a worthless wreck, was towed to the New-Jersey side of the river.

The steamer had two 'flue-boilers' 25 feet (7.6 m.) long, 6 feet 4 inches (1.93 m.) in diameter, containing four 'direct' flues 14 inches (0.36 m.) in diameter, two of 9 inches (0.23 m.) diameter, and five 'return' flues of 11½ inches (0.28 m.) diameter. The shell was of no. 3 iron, and the area of heating-surface was 676 square feet (63 sq. m.). The iron was of good quality, and was in good condition throughout, except along the bottom, where it gave way. The form, proportions, and workmanship of the boiler were good. The builders, Messrs. Fletcher & Harrison of New York, were among the most reputable constructors of engines and boilers in that city, and were noted for doing good work.

On examination, it was found that the bot-

tom was corroded along its whole length, and had been patched in a number of places where the iron had become dangerously thin, and that in some places the sheets were reduced to one-fourth their original thickness. The shell had been repeatedly patched, and five 'soft patches' were found on the girth-seams. The rupture seems to have started in the thin parts of the bottom, and to have followed the weakened girth-seams quite around, and divided the mass into two parts of nearly equal size, tearing the middle sheet out of the shell entirely.

A coroner's jury made an inspection, examined such witnesses as could be found and such experts as could be induced to testify, and rendered a verdict to the effect that the boiler ruptured in consequence of the weakness of the sheets on the bottom of the shell, which were unable to sustain the working pressure allowed by the U. S. inspector; which weakness had been produced by corrosion on the interior, due to the action of the feed-water. It was further asserted, that the boiler was tested in June up to a pressure of 62 pounds (5 atmos. abs.), and burst ten weeks later under a pressure of but 32 pounds (3 atmos.), in consequence of the neglect of the inspector to observe its condition at the time of testing it. The engine-driver and the inspector were censured by a vote which was not unanimous. The jury expressed the opinion that the present law is not sufficiently explicit and mandatory, and that the use of the test by hydrostatic pressure is insufficient to detect and reveal such defects as here existed.

The inspector acknowledged that he did not try the strength of the boiler with the hammer, as is now usual in all thorough examinations by competent engineers, but merely looked it over; and that at previous inspections he had not entered the boiler, but had only looked in at the manhole. The evidence of the most superficial and inefficient 'inspection' was conclusive; and the fact that proper inspection would have revealed the dangerous condition of the boiler was equally well proven. The so-called 'inspection' was a farce; and the inspector, in a spirit of indifference or indolence, took the chances of an explosion.

The exploded boiler weighed 27,000 pounds (12,247 kilos), and contained 25,000 pounds (11,340 kilos) of water. The explosion was not remarkably violent, but was what old engineers are accustomed to call a 'burst' rather than an explosion. The consequences were, however, sufficiently serious. The energy producing the effects seen in the case of a

true explosion may be imagined, when the amount of heat-energy stored up in such a boiler is calculated. The quantity of heat transformed into mechanical energy by a mass of water and of steam of such magnitude, set free, and expanding down to the pressure and temperature of the atmosphere, from the pressure and temperature at which it existed in the boiler of the Riverdale, would amount to above 1,500,000,000 foot-pounds (over 200,000,000 kilogrammetres). This would be sufficient to throw the boiler and its contents, were the heat all utilized, as in a perfect steam-engine, five miles high. This may give some faint idea of the enormous forces at work, and the tremendous energy stored in a steam-boiler, even where the pressure of the steam is very low, as it was in this case.

It will be concluded, from what has been above stated, that a steam-boiler of the most ordinary and least dangerous type has stored within it an inconceivable amount of available energy in the form of heat, which may be at any moment transformed, in part, into mechanical energy with terribly destructive results, both to life and property; that this powerful agent for good or for evil can only be safely utilized when the utmost care, intelligence, and skill, are employed in its application, and in the preservation of the vessel in which it is enclosed; that the present code of law relating to the care, management, and inspection of steam-boilers, is entirely inadequate to insure safety; that the inspection of steam-boilers, as at present practised by the employees of the government, is not only liable to be inefficient, but is likely to prove worse than none, as it gives to the owner, and perhaps often to the man in charge, of the boiler, a feeling of security which is entirely without basis in fact, and which may therefore cause the neglect of that watchfulness which might otherwise prevent accident; that simple pressure produced by the test-pump, as now provided for by the law, is not a sufficiently effective method of detecting weakness in the boiler, or to be relied upon to the exclusion of other better and well-known methods of test.

The fact that the hydrostatic test is not conclusive as to the safety of a boiler has long been well known and admitted among intelligent engineers. The steam ferryboat Westfield met with precisely such an accident a dozen years ago; and it was shown at the coroner's inquest, at which the writer assisted that official in the examination of his expert witnesses, that the boiler had been inspected, and had been tested, but a few weeks before, by the

U.S. inspector, who applied a pressure considerably in excess of that at which the explosion took place. The cause of the accident, by which a large number of people lost their lives, was precisely that which caused the explosion of the Riverdale's boiler, and the method of rupture was the same. In either case, proper methods of inspection would have saved the lives of the sufferers.

It is undoubtedly true, that many of the inspectors are conscientious, experienced, skillful, and painstaking men, and do their duty in spite of the defects of the existing law; but it is also true that now and then a careless or incompetent inspector will neglect the simplest details of his work, and that we must expect occasional repetition of this sad experience, until the law is intelligently framed, and so administered that the passing of a defective boiler by the inspector shall become as nearly as possible an impossibility.

ROBERT H. THURSTON.

Hoboken, Sept. 23, 1883.

THE AMERICAN SOCIETY OF MICROSCOPISTS.

THE sixth annual meeting was held this year in Chicago, Aug. 7-11. The usual number of members was present, and the meeting was full of interest from the beginning to the end. The forenoon session of Tuesday was given to organization, and the report of the president on the official action of the executive officers for the year. At the afternoon session, papers were read as follows. Microscopical examination of seminal stains on cloth, by F. M. Hamlin. After pointing out the defects of Koblanck's method, that usually given in the manuals, he explained his own, which he had found eminently successful. It is in brief as follows. "1. If the stain to be examined is upon any thin cotton, linen, silk, or woollen fabric, cut out a piece about one-eighth inch square, lay it upon a slide previously moistened with a drop of water, and let it soak for half an hour or so; . . . then with a pair of needles unravel or fray out the threads at the corners, put on the glass cover, press it down firmly, and submit to the microscope. 2. If the fabric is of such a thickness or nature that it cannot be examined as above, fold it through the centre of the stain, and with a sharp knife shave off the projecting edge thus made, catching upon a slide moistened with water the particles removed. After soaking a few minutes, say five to ten, the powdery mass will sink down through the water, and rest upon the slide. The cover-glass may now be put on, and the preparation examined."

College microscopical societies, by Sarah F. Whiting. The author discussed, first, the question 'What use can a microscopical society subserve?' second, 'How can it be made a success?' Such a society, in its range of topics, can take in almost all the physical

and social sciences; and an enthusiasm created in it will help many of the departments of college-work. The society welcomes the incoming classes, and affords an opportunity for a change to those accustomed to years of dry drill in the mathematics and languages of the preparatory schools. It opens their eyes to the perfections of nature, awakens in them a curiosity, and stimulates interest in all the scientific studies of the college course. There are interesting subjects of microscopical investigation, carried on in society, which do not come under any department of college instruction. The live college microscopical society will not only awaken interest in students and teachers, but will attract the attention of the authorities who control the funds, and the community generally. The conditions of success in such a society are just what they are in every other scientific society the object of which is not alone investigation, but instruction of its members and others. There must be desirable members, those who are willing to work: all must have something to do. "The freshmen, by their exclaiming, fan the flame of enthusiasm, and, before they can do any thing alone with the microscope, can serve the society on the lamp committee." There should be a class in microscopical technology, apparatus, a library, and especially the current scientific journals.

Cataloguing, labelling, and storing microscopical preparations, by Simon H. Gage. This paper pointed out the advantages of properly cataloguing, etc., one's microscopical preparations, and then gave in detail the course found to be successful by the author. "The catalogue should indicate all that is known of a specimen at the time of its preparation, and all the processes by which it is treated. It is only by the possession of such a complete knowledge of the entire history of a preparation that one is able to judge with certainty of the comparative excellence of methods." The card method is advocated. "The cards are postal-card size, and each preparation has its own card. . . . These may be arranged alphabetically; and, as new preparations are made, new cards may be added in their proper order, while those of destroyed or discarded preparations may be removed without in any way marring the catalogue. Finally, the cards may be kept in a neat box which occupies but little more space than a manuscript book." The cabinet should allow the slides to be flat, exclude dust and light. Each slide should have a separate compartment, numbered to agree with the slide. The floor of the compartment should be beveled at the end, so as to facilitate removal; and the drawers of the cabinet should be independent, but so close together that slides will not fall out when tipped; and each should be numbered with Roman letters.

The president, Albert McCalla, delivered his annual address, Tuesday evening, in Weber music-hall. His theme was 'Verification of microscopical observation.' Referring to the scientific spirit and a common bond of the society's organization, he said, "In this intensely practical age of ours, we are in danger of forgetting that the true aim of science is simply the pursuit of truth, and that the mighty

benefits, the invaluable and almost countless gifts, of wealth and ease and safety, which result from scientific discovery, and which so greatly bless the world to-day, will result most surely when science has an eye single to the search for simple truth, — truth that to the practical world seems often abstract and unimportant. . . . We are not all botanists, not all zoölogists, nor all students of lithology; yet we have a well-defined common ground. We are all deeply interested in the physics of the microscope and in the methods of its use; and, in order to be skilled in that department of investigation we have severally chosen, we must be more or less fully practical in microscopical work in many fields." To prove that "only as theories are submitted to repeated and varied forms of verification is error eliminated and final truth obtained," numerous facts were cited from the history of scientific progress; after which, the means of verification of microscopical observation were discussed. These are repetition of observation, use of the camera lucida, photomicrography, media and reagents, improved lenses and apparatus, and a better knowledge of optics.

Wednesday forenoon was devoted to papers on bacteria. T. J. Burrill read a paper on preparing and mounting bacteria. He stated that the elements of successful staining are as follows: "1. The organisms should be decidedly and conspicuously stained; 2. The general mass of embedding-material should be left unstained, or so different in color that the organism can be distinctly seen; 3. There should be no granular or other precipitations from the staining-material; 4. The color should be suitable for the purposes required, and permanent if the object is to be mounted; 5. The process should be as simple as possible, and free from manipulative difficulties. . . . Except for a few special results, aniline dyes are by far the most serviceable in staining bacteria. However, no one staining-material, nor any single method of procedure, can be made to answer well the requirements for all kinds of bacteria."

Dr. H. J. Detmers presented some conclusions reached by himself while studying the diseases of cattle in Texas. Bacteria he regarded as unquestionably the cause of certain fevers. Under certain conditions, all bacteria become pathogenetic; but these conditions are not yet fully understood.

Dr. George E. Fell read a paper on the clinical advantages of ozone, and its effects on the micro-organisms of infusions. After giving the favorable results of the use of ozone by Dr. F. W. Bartlett in scarlet-fever, diphtheria, whooping-cough, typhoid-fever, etc., he gave in tabular form the results of a series of experiments by himself, — experiments in which bacteria, and other forms indigenous in infusions, were subjected to the influence of air charged with ozone.

Following the discussion on the three preceding papers, Dr. G. E. Blackham presented the report of committee on oculars. The report recommends naming oculars, like objectives, by their equivalent focal lengths in English inches. For the tubes of oculars

the standard medium size, 1.25 inch is recommended, with the alternatives of 1 inch and 1.35 inch for those who wish smaller or larger tubes; also for the upper tube of the ocular .75 inch, and, for sub-stage tube, 1.50 inch. The purpose of the society is to secure uniformity in these sizes, so that apparatus of different makers shall be interchangeable, as objectives have been since the adoption of the 'society screw.'

The first paper in the afternoon was a critical study of the action of a diamond in ruling lines on glass, by Prof. William A. Rogers. The author stated his theory relating to the method which Nobert may possibly have employed in the production of his plates briefly thus: "The lines composing Nobert's finest bands are produced by a single crystal of the ruling-diamond, whose ruling qualities improve with use. . . . When a diamond is ground to a knife-edge, this edge is still made up of separate crystals, though we may not be able to see them; and a perfect line is obtained only when the ruling is done by a single crystal. When a good knife-edge has been obtained, the preparation for ruling consists in finding a good crystal. Occasionally excellent ruling-crystals are obtained by splitting a diamond in the direction of one or more of the twenty-four cleavage planes which are found in a perfectly formed crystal. A ruling-point formed in this way is, however, very easily broken, and soon wears out. Experience has shown that the best results are obtained by choosing a crystal having one glazed surface, and splitting off the opposite face. By grinding this split face, a knife-edge is formed against the natural face of the diamond, which will remain in good condition for a long time. When a ruling-crystal has been found which will produce moderately heavy lines of the finest quality, it is at first generally too sharp for ruling lines finer than 20,000 or 30,000 to the inch, even with the lightest possible pressure of the surface of the glass. But gradually the edges of this cutting-crystal wear away by use, until at last this particular crystal takes the form of a true knife-edge which is parallel with the line of motion of the ruling-slide: in other words, when a diamond has been so adjusted as to yield lines of the best character, its ruling-qualities improve with use. If Nobert had any so-called 'secret,' I believe this to have been its substance.

"The problem of fine ruling consists of two parts, —first, in tracing lines of varying degrees of fineness; and, second, in making the interlinear spaces equal. The latter part of the problem is purely mechanical, and presents no difficulties which cannot be overcome by mechanical skill. It will be the aim of the present paper to describe the more marked characteristics of lines of good quality ruled upon glass. . . . A perfect line is densely black, with at least one edge sharply defined. Both edges are perfectly smooth. Add to these characteristics a rich black gloss, and you have a picture of the coarser lines of a perfect Nobert plate. In the study of the action of a diamond in producing a breaking fracture in glass, the microscope seems to be of little service; but we can call it to our aid in the study of its action in ruling smooth lines. One would naturally suppose that a line

of the best quality would be produced by the stoppage of the light under which it is viewed by the opaque groove which is cut by the ruling-diamond. Without doubt, this is the way in which lines are generally formed; but it is not the only way in which they can be produced. An examination under the microscope will reveal the fact, that in some instances, at least, a portion of the glass is actually removed from the groove cut by the diamond; and that the minute particles of glass thus removed are sometimes laid up in windrows beside the real line, as a plough turns up a furrow of soil. . . . The particles of glass removed take four characteristic forms: (a) They appear as chips scattered over the surface of the glass; (b) They appear as particles so minute, that when laid upon a windrow, and forming an apparent line, they cannot be separated under the microscope; (c) They take the form of filaments when the glass is sufficiently tough for them to be maintained unbroken; (d) They take a circular form. . . . It must not, however, be supposed that lines of the best quality always present the appearance described above." These characteristic results were illustrated by plates, with the fragments and fibres in place. The author also referred to Mr. Fasoldt's claim, that he has succeeded in ruling lines one million to the inch. He thinks the limit just a trifle too high; but, if reduced one-half, he is by no means sure but that it may be reached.

Following this was a paper by A. H. Chester, describing a new method of dry mounting, in which the cover-glass is easily removable. The object is fastened to the slide in the usual way, and a tin cell built up about it sufficiently high for the cover to clear the object; then a ring of larger bore is cemented on, making a flange to receive cover. The latter ring, having been punched out of tin foil by a gun-wad punch, and put in place with the smaller hole uppermost, makes a groove above the cover, into which a spring-brass ring is put, holding the cover in place. The advantages of this method are many, — the objects may be easily viewed uncovered; dust or moisture accumulating in the cell may be removed; the mounting is quickly done, etc.

In the evening session, Mr. W. H. Walmsley read a paper giving the latest and best results and methods, by himself and others, in the art of photomicrography. The photographs in illustration were exhibited by L. D. McIntosh by means of his solar microscope with ether oxygen light.

Following this was a short paper by D. S. Kellicott, giving an account of certain stalked infusoria found in the crayfish. Two species, believed to be peculiar to the gill-chambers, were described. One of these is new to science, and was named *Cothurnia variabilis*. The well-marked varieties seem to be due to situation; i.e., those on the hairs of the lining-membrane are relatively longer, with a spine at the upper edge of the aperture of the lorica, but without a spine on the ventricose front of the same, while a stouter variety, situated on the membrane itself, has a spine in front, but none on the edge of the aperture. A third variety occurs on the gills: it also has a spine

on the ventricose part of the shell, but the aperture is horizontal instead of vertical. They are so abundant as to encumber the gills of the host, rendering them brown to the eye. An *Epistylis* from the same host was named *E. Niagaræ*: it is close to *E. balanorum*, a marine form. The same writer, at another session, described *Cothurnia lata* n. s., found on *Diaptomus*, and also gave a general account of two internal parasites of the crayfish.

The first paper Thursday morning was on the effects of division of the vagi on the muscles of the heart, by A. M. Bleile. The object of the paper was the demonstration of nutritive or trophic nerves for the heart, and was a continuation of work reported in the Proceedings of the society last year.

Following this, T. S. Up de Graff described certain fresh-water worms. One, a rotifer, is new: it was named *Brachionus Gleasonii*. Independent of the spines, its length is 0.145 inch, the front of the carapace without spines. The posterior edge bears five curved spines: there is one, also, on the dorsal part of the shell, — a peculiar feature.

The remainder of the session was occupied by short papers, by Francis Wolle, on fresh-water algae, and one by John Kruttschnitt on ferns and their development.

In the afternoon the session was opened by a report upon a standard centimetre, prepared by the U. S. bureau of weights and measures, by W. A. Rogers. The lines of the centimetre are ruled on a plate of platinum-iridium soldered to brass with silver. The report describes the plate, and compares its divisions with a standard. The original basis of this unit is a metre upon copper, prepared for Professor Rogers by Professor Tresca of the Conservatoire des arts et métiers at Paris. The report concludes thus: —

1. That the centimetre *A*, measured by the middle defining line, is exactly a hundredth part of the metre des archives reduced to sixty degrees Fahrenheit. It can therefore be safely adopted as the unit in all measures with low-power objectives.

2. That the second millimetre of the scale is exactly a thousandth part of the metre des archives when at the same temperature.

The centimetre is now the property of the society; it having been tendered to it by the national committee on micrometry, and accepted and adopted as a standard or basis for future studies and discussions in micrometry. The scale is in the hands of Dr. George E. Fell. A committee was appointed for securing copies on glass. The rules for its control and use will soon be published.

After this report, Dr. George E. Blackham read a communication on the relation of aperture to amplification in the selection of a series of microscopic objectives. The author showed that amplification is not the only element which enters into the problem of rendering visible minute details, but the aperture of the objective forms another element. Working, then, on the general lines laid down, he had selected as a set of powers, sufficient for all the work of any microscopist, the following: —

One 4-inch objective of 0.10 n.a. = 12° an angle, nearly.

" 1 "	" "	" 0.26 n.a. = 36° "	" "
" $\frac{1}{6}$ "	" "	" .94 n.a. = 140° "	" "
" $\frac{1}{8}$ "	" "	" 1.42 n.a.	

"The first two to be dry-working objectives, without cover-correction; the third to be dry-working, with cover-correction; and the fourth to be a homogeneous immersion objective, with cover-correction; and all to be of the highest possible grade of workmanship. The stand should have a tube of such length that standard distance of ten inches from the front surface of objective to diaphragm of eye-piece can be obtained on it, and to be furnished with six eye-pieces; viz., 2-inch, 1-inch, and $\frac{3}{4}$ -inch Huyghenian, and $\frac{1}{2}$ -inch, $\frac{1}{3}$ -inch, and $\frac{1}{4}$ -inch solid."

In the evening the annual *soirée* took place, in connection with the State microscopical society of Illinois, at the Calumet club-house. There were two hundred and fifty microscopes on the tables. A great variety of objects were shown to a party of five hundred guests of the club.

There were a number of reports and papers to be disposed of on Friday, the society adjourning at five P.M.

The first paper was by Dr. W. T. Belfield, on the detection of adulteration in lard. Photographs representing crystals of pure and adulterated lard were exhibited. Those of the former are long, thin plates, with beveled ends, while the crystals of tallow are plume-shaped, resembling somewhat an ostrich feather.

Dr. V. S. Clevinger presented a paper on the pathology of the brain.

Dr. Thomas Taylor's paper, on internal parasites in the domestic fowl, was read. The parasites referred to were a mite from the lungs, another mite in the cellular tissue, and an encysted nematoid from the crop.

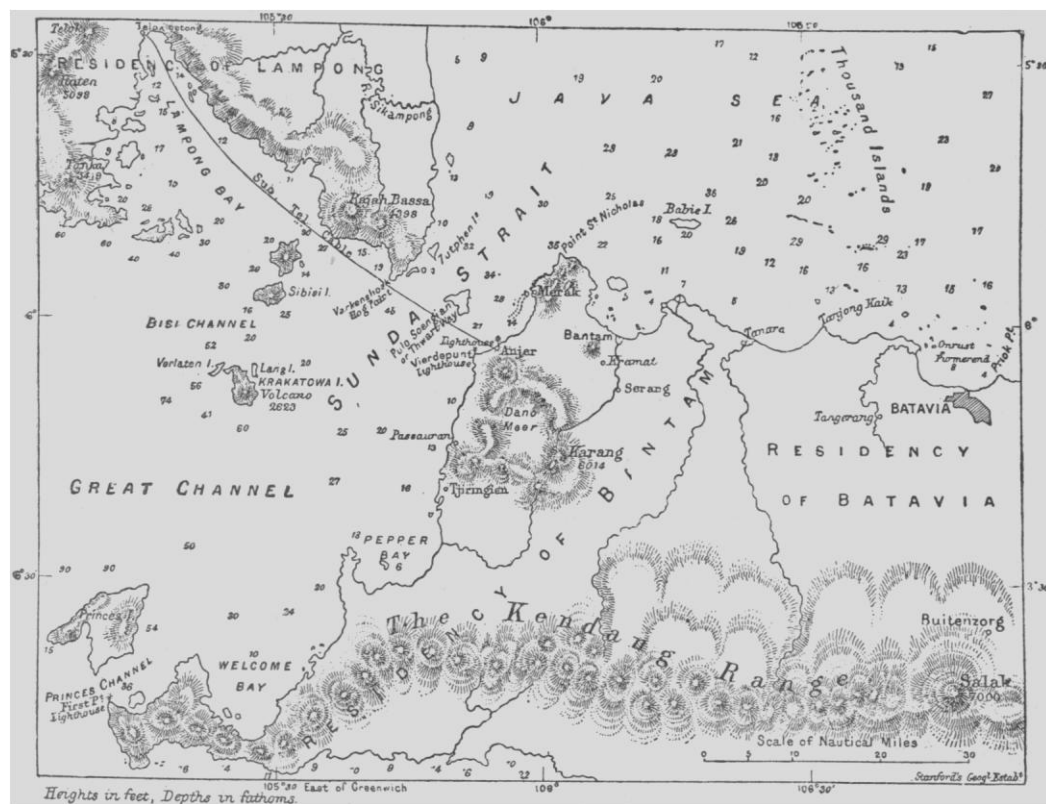
A paper on the termination of the nerves in the kidneys was presented by M. L. Holbrook. The author's method may be stated thus: "The fresh kidneys, as well as those preserved in chromic-acid solutions, were frozen in the freezing-microtome of Dr. Taylor, and the sections allowed to remain in the gold solution for varying periods of time, from forty minutes to several days. When removed, they were carefully washed in distilled water, and placed in a strong formic acid of a specific gravity of a hundred and twenty degrees for from five to eight minutes, or in a twenty-five-per-cent solution of the same for hours and even days. Sometimes I obtained very good specimens by placing the sections first in a dilute twenty-five-per-cent solution of formic acid for twenty-four hours, and afterward staining them with chloride of gold until they reached the color desired. . . . The nerves supplying the kidneys are mainly of the non-medullated variety. They accompany the larger arteries of this organ, either in bundles, or in flat, expanded layers; and the latter features I found more common than the former. Sometimes an artery would be found encircled by a network of non-medullated nerves in a bewildering number. Hundreds of such nucleated bundles of fibres could be traced

around, above, and below an artery, freely branching, bifurcating, and supplying all the neighboring formations with a large number of delicate fibrillae. . . . The bundles of nerve-fibres give off delicate ramules to the afferent vessels by which they enter the tuft; and here they produce a delicate plexus spun around its capillaries. It was impossible to decide where the ultimate fibrillae branch in the capillaries of the tuft. . . . Sometimes I obtained specimens in which it seemed as if the ultimate fibrillae branched beneath the covering, flat epithelia in the delicate connective tissue between the convolutions of the capillaries. . . . In perfect specimens there is no difficulty in

the cat; and Dr. F. M. Hamlin, on mounting foraminifera. New apparatus was described as follows: new microscope-stand with concentric movements, by J. D. Cox; new modification of the Spitzka microtome, by V. S. Clevinger; and a new binocular arrangement, by Edward Bausch.

The next annual meeting will be held at Rochester, N.Y., in August, 1884.

The officers for the present year are: president, Hon. Jacob D. Cox; vice-presidents, William A. Rogers, T. J. Burrill; secretary, D. S. Kellicott; treasurer, George E. Fell; executive committee, Albert H. Chester, William Humphreys, and H. A. Johnson.



satisfying one's self of the fact that every tubule is encircled by a plexus of non-medullated nerve-fibres, coursing either in the immediate vicinity of the tubule, in the interstitial connective tissue, or within the dense layer subjacent to the epithelia, known as the *membrana propria*, or even with the layer along the feet of the epithelia themselves."

Short papers were read by Dr. Salmon Hudson, on the yeast-plant; by J. M. Mansfield, on division of labor among microscopists; by Dr. L. M. Eastman, on egg-like bodies in the liver of the rabbit; by Dr. George E. Fell, on a peculiarity in the structure of the human spermatozoon. Dr. Lester Curtis made some observations on vessels of the spinal cord of

THE JAVA UPHEAVAL.¹

THE details which have reached us during the past week, of the terrible seismic manifestation at Java, prove it to be one of the most disastrous on record. Probably, moreover, it is the greatest phenomenon in physical geography which has occurred during at least the historical period, in the same space of time. The accompanying sketch-map will afford some idea of the extent and nature of the change which has taken place, and the character of the sea-bed and the land in the region affected.

The volcanic island of Krakatoa lies about the

¹ Taken from *Nature*, Sept. 6.

middle of the north part of the passage between Java and Sumatra, a passage which has formed an important commercial route. The strait is about seventy miles long and sixty broad at the south-west end, narrowing to thirteen miles at the north-east end. The island, seven miles long by five broad, lay about thirty miles from the coast of Java; and northwards the strait contracts like a funnel, the two coasts in that direction approaching very near to each other. A few weeks ago, as we intimated at the time, the volcano on the island began to manifest renewed activity. The whole region is volcanic; Java itself having at least sixteen active volcanoes, while many others can only be regarded as quiescent, not extinct. Various parts of the island have been frequently devastated by volcanic outbursts, one of the most disastrous of these having proceeded from a volcano which was regarded as having been long extinct. The present outburst in Krakatoa seems to have reached a crisis on the night of Aug. 26. The detonations were heard as far as Soerakarta; and ashes fell at Cheribon, about 250 miles eastwards on the north coast of Java. The whole sky over western Java was darkened with ashes; and, when investigation became possible, it was found that the most wide-spread disaster had occurred. The greater part of the district of North Bantam has been destroyed, partly by the ashes which fell, and partly by an enormous wave generated by the wide-spread volcanic disturbance in the bed of the strait. The town of Anjer, and other towns on the coast, have been overwhelmed and swept away; and the loss of life is estimated at 100,000. The island of Krakatoa itself, estimated to contain 8,000,000,000 cubic yards of material, seems to have been shattered, and sunk beneath the waters; while sixteen volcanic craters have appeared above the sea between the site of that island and Sibisi Island, where the sea is comparatively shallow. The Soengepan volcano has split into five; and it is stated that an extensive plain of 'volcanic stone' has been formed in the sea, near Lampong, Sumatra, probably at a part of the coast dotted with small islands. A vessel near the site of the eruption had its deck covered with ashes eighteen inches deep, and passed masses of pumice-stone seven feet in depth. The wave reached the coast of Java on the morning of the 27th, and, thirty metres high, swept the coast between Merak and Tjiringin, totally destroying Anjer, Merak, and Tjiringin. Five miles of the coast of Sumatra seem to have been swept by the wave, and many lives lost. At Taujong Priok, fifty-eight miles distant from Krakatoa, a sea seven feet and a half higher than the ordinary highest level suddenly rushed in, and overwhelmed the place. Immediately afterwards it as suddenly sank ten feet and a half below the high-water mark, the effect being most destructive. We shall probably hear more of this wave, as doubtless it was a branch of it which made its way across the Pacific, and that with such rapidity that on the 27th it reached San Francisco harbor, and continued to come in at intervals of twenty minutes, rising to a height of one foot for several days. The great wave generated on May 10, 1877, by the earthquake at

Iquique, on the coast of Peru, spread over the Pacific as far north as the Sandwich Islands, and south to New Zealand and Australia; while that at Arica, on Aug. 13 and 14, 1869, extended right across the Pacific to Yokohama (*Nature*, vol. i. p. 54). It is misleading to speak of such waves as tidal: they are evidently due to powerful, extensive, and sudden disturbances of the ocean-bed, and are frequently felt in the Pacific when no earthquake has been experienced anywhere, though doubtless due to commotions somewhere in the depths of ocean. So far, these are all the facts that are known in connection with this last stupendous outburst of volcanic energy. It has altered the entire physical geography of the region, and the condition of the ocean-bed. The existing charts of the strait, with their careful soundings, are useless for purposes of navigation; and, when quiescence is restored, a new series of soundings will be necessary. Doubtless the results of the outbreak will receive minute attention at the hands of the Dutch government; and, when all the data are collected, they will form valuable material for the study of the physical geographer.

LETTERS TO THE EDITOR.

Humblebees vs. field-mice.

IN SCIENCE of Sept. 7 the vice-president of section F (biology), in his address of Aug. 15, referring to the aid given by humblebees in fertilizing *Trifolium pratense*, is reported as saying, "Humblebees prefer to raise their colonies in old nests of meadow-mice. I mentioned in my last report, that it had been suggested that we should not keep many cats, nor allow hawks, foxes, or dogs to catch these mice; for they make nests which are quite necessary for the humblebees, which help fertilize our red clover, and thereby largely increase the yield of seed."

I would beg leave to differ from the author of the suggestion referred to, on the ground, that, if carried out, the effect produced would be apt to be quite the contrary of that intended. As field-mice prey upon the nests and combs of the humblebees, acting as a great check to their increase in numbers, the greater the precautions taken to prevent the killing of the mice, the greater would be the tendency towards the extermination of the humblebee, and therefore the less would be the yield of seed, resulting from the lack of aid rendered by these insects in fertilizing the red clover.

In support of my objection, I would refer to Darwin's *Origin of species*, sixth edition, third chapter, where, under the head of "Complex relations of all animals and plants to each other in the struggle for existence," he says, "The number of humblebees in any district depends in a great measure on the number of field-mice, which destroy their combs and nests; and Col. Newman, who has long attended to the habits of humblebees, believes that 'more than two-thirds of them are thus destroyed all over England.'" E. NUGENT.

Pottstown, Penn., Sept. 15, 1883.

The influence of winds upon tree-growth.

I observe that in the vicinity of Cambridge and Boston, wherever the common New-England elms stand in a moderately isolated site, and one exposed to the wind, they lean, in a large majority of cases, trunk and all, to the south-east. This is true, also,

to a less extent, of maples; but the oak, ash, poplar, and pine stand sturdily erect. I believe the leaning of the elastic-fibred elms is due to the prevailing winds, which are from the west and north-west, these winds being also the strongest and coldest. At the office of the U. S. signal-service in Boston, observations are taken three times a day. In 1882, out of 1,095 observations taken, 298 showed the wind to be in the west, and 225 showed it to be in the north-west: in other words, about half (or forty-seven per cent) of the observations showed the wind to be somewhere between west and north-west. For the other five years the record is as follows:—

1877.	1878.	1879.	1880.	1881.
247 W.	229 W.	273 W.	301 W.	278 W.
169 N.W.	231 N.W.	257 N.W.	153 N.W.	175 N.W.

So much for the prevailing direction of the wind. There seems to be no other cause than this, to which we can assign the phenomenon of growth in question. All the many exceptions to the rule are to be explained, doubtless, by local causes, — shelter, neighborhood of other trees, and other more occult conditions of fibre. The works on forestry and botany seem not to notice the fact of asymmetry in tree-growth. It is only a repetition, on a larger scale, of the graceful deviation from monotonous symmetry which characterizes all leaf and branch structure.

W. S. KENNEDY.

Importance of lime-juice in the pemmican for arctic expeditions.

The recent failure to relieve the party under Lieut. A. W. Greeley at Lady Franklin Bay leads us to recur to the repeated difficulties which have marked the history of former arctic expeditions. We have re-examined the accounts of the English expedition of the Alert and the Discovery, under Nares and Stephenson, which left England, May 29, 1875. It was the first English arctic expedition which had orders to endeavor to reach the North Pole. It had the advantage of the advice of experienced arctic navigators, its commander Nares having been a member of several such expeditions.

Thus it surprises the reader, that more thorough precautions were not made against the scurvy. The *London quarterly review* for January, 1877, has the fullest account of the ravages committed by that disease with the sledge-parties sent out by Nares. Of the sledge-party under Commander Parr it says,—

"Of seventeen of the finest men of the navy, who composed the original party, but five were (on return) able to walk along-side. One was dead, and the remainder in the last extremity of illness."

It gives a minute account of the prostration by scurvy of the two other sledge-parties, — one under Commander Beaumont, and one under Commander Aldrich. Concerning the latter, the *Review* says, —

"To quote from the journal of Commander Aldrich, who led the western division, would be to repeat the same dreadful details. The party broke down, and were supported by the same pluck, and brought back alive — that is all one can say — by the help of God and the same determined courage. Surely, nothing finer was ever recorded than this advance of three sledges, — one to the north, another to the east, a third to the west, — laden down with sick and dying men, in obedience to an order to do their best, each in their separate direction. It is the old story, — too common in English annals, — the organization broke down, and individual heroism stepped in to save the honor of the day. But at what a cost!"

All this was because the parties had no lime-juice. And Capt. Nares, "with a chivalry and candor which do him honor, whether he has failed in judgment or not, declared that such was the fact, and that the omission was made by his orders and on his responsibility." He said, —

"Acting on my lights and experience at the time, I followed the example of such men as M'Clintock, Richards, Michan, and McClure, of the Investigator, and started off our sledges with as nearly as possible the same rations as had proved fairly successful on all previous occasions; that is, without lime-juice for issue as a ration, a small quantity for use as a medicine being carried by the sledges, which were not expected to be able to obtain game. . . . Up to the middle of May the lime-juice remains as solid as a rock. No sledge-party employed in the arctic regions in the cold month of April has ever been able to issue a regular ration of lime-juice. In addition to the extra weight to be dragged, that its carriage would entail, there is the even more serious consideration of the time necessary in order to melt sufficient snow."

He added, —

"Of course, hereafter, lime-juice in some shape or other must be carried in all sledging journeys; and we earnestly trust that some means will be found to make it in a lozenge, for, as a fluid, there is, and will always be, extreme difficulty in using it in cold weather, unless arctic travelling is considerably curtailed."

The *Quarterly review*, in quoting these manly remarks of Capt. Nares at Guildhall, says, —

"Even if it should be found that Sir George failed in judgment in this matter, he has in our opinion shown the finer form of fitness for command, in his readiness to assume the responsibility of his acts."

His frankness and manliness in assuming the whole blame to himself have evidently, in great measure, disarmed criticism.

But this brings us to the main object in this letter; and that is, to recur to the remedies which this story has suggested. If lozenges of lime-juice in a shape for arctic exploration have not been manufactured, they certainly can at least now be found at the druggists in a shape to be used as troches for colds.

But the efficient remedy is to have pemmican made which is permeated with lime-juice, as recommended in the 'Report of the surgeon-general of the navy for 1880' (see p. 356). Gen. P. S. Wales said, —

"The indispensable necessity of lime-juice in the sledging-parties, and the difficulties of carrying it, and preparing it for use, induced me to suggest the propriety of combining the juice and pemmican in the proportion of one ounce to the pound of the latter. The pemmican is greatly improved in taste and flavor, and will, I believe, be more assimilable. This is an important modification, as there are persons who cannot eat the ordinary article."

The article was prepared as proposed, and tried in Washington, and pronounced to be very palatable.

Gen. George H. Thomas, in preparing for one of his battles, issued a general order, enjoining upon his whole army strict attention to minutiae, saying that "the loss of a battle might be due to one missing linchpin."

In recurring to this recommendation from the office of the surgeon-general of the navy, we have thought that it may be considered opportune, when the minds of many are now turned upon the arctic expeditions. We think that recommendation was followed, so far as the preparations of the Jeannette and the Rodgers were concerned; but, alas! they never got so far as to turn their attention to fitting out explorations with sledge-parties.

BENJAMIN ALVORD.

Rensselaeria from the Hamilton group of Pennsylvania.

Will you kindly afford me a small space to correct an error in your report of the discussion which followed the reading of my paper at Minneapolis? On p. 327 of your issue for Sept. 7 occur the following sentences: —

"The differences between them [the fossils exhibited and the Oriskany species of *Rensselaeria*] were slight, though well marked. Professor Hall described some of these differences, and Mr. Claypole acknowledged that a certain V-shaped groove was wanting in his specimens. Professor Hall thought that possibly the fossils should be referred to *Amphigenia*, which had many similarities to *Rensselaeria*."

The V-shaped groove in question is one of the generic marks of *Amphigenia*; and its absence, therefore, was urged by me as excluding the fossils from that genus, and inferentially as a strong argument in favor of placing them in *Rensselaeria*.

As the above-mentioned error places me (and I think Professor Hall also) in false positions, and involves a grave mistake in paleontology, I am induced to ask your insertion of this correction, which I have submitted to Professor Hall for his approval.

I ought to add that the suggestion of *Amphigenia* by Professor Hall was only the result of a momentary impression on the first sight of the fossil, and one which he immediately withdrew, on observing the absence of the V-shaped groove above alluded to.

E. W. CLAYPOLE.

Aurora.

The auroral display here to-night was unusually brilliant. I observed it first at 7.04 P.M. At this time a low but rather brilliant arch of light spanned the north-eastern horizon, the crest of the arch having an altitude of about 20°. During the next three minutes, the lights rapidly took on the 'streamer' form, gradually shooting upward to a little beyond the zenith, and at this time stretching from east, 10° south, around to west, 15° north, on the horizon. During about two minutes, the waving-curtain aspect was very pronounced in the north-east, after which only striated patches flamed out here and there, moving alternately west and east. These patches all converged toward the zenith, but left with one the impression of being pendulous and very near. The atmosphere appeared very clear, the moon full and bright, the twilight still strong; and there was light enough yet to enable one to read a newspaper, but with difficulty. The streamers, however, lay in sharp contrast against the blue sky, even where the twilight was strongest.

At 7.15 the lights began to die rapidly away, and at 7.50 none were to be seen; but at 8, and again at 8.13, there were distinct but small curtains to be seen in the north-west. At 8.20 there began a magnificent display. Three large curtains formed one above the other, the lowest about 20° above the horizon in the north-west. They drifted gently toward the zenith, swaying and folding just enough, it seemed, to suit the almost imperceptible breeze which was stirring. The lights could be easily seen within 7° of the moon; and yet it cast its shadow on the carpet in a room 13 by 14, where two kerosene-lamps were burning, one of them a no. 1, and the other a no. 2, burner. At 9.10 scarcely a trace of the aurora could be seen. A little later, a very faint diffuse light covered the northern sky to an altitude of about 25°. This soon became striped, and afterwards appeared to move bodily toward the zenith. At 10.20 the lower sky had become a deep blue; and just above it, at an altitude of 30°, a broad arch of bright but uniform light formed across the sky; and above this, extending past the zenith, were similar but much fainter bands. Five minutes later, the bright band unfolded a curtain which dropped in exquisite folds toward the horizon. This lasted less than two minutes, the whole belt of light becoming striated, but leaving a clear space next

to the horizon; then followed about five minutes during which the illuminated portion of the sky seemed to be throbbing, and sending out waves of subdued light, which spread southward over the blue vault, dying away before the zenith was reached. This movement soon became more violent; and between 10.40 and 10.45 the lights had more the appearance of flames bursting rapidly from the sky, and spreading to the zenith, where they often turned abruptly toward each other, and met. This appearance continued growing gradually less marked until 12.15 A.M., when there was scarcely a trace of auroral display. At 12.40 a faint arch of diffuse light could be seen in the north, like that already described.

F. H. KING.

River Falls, Wis., Sept. 16, 1883.

THOMPSON'S PHILIPP REIS.

Philipp Reis: inventor of the telephone. A biographical sketch, with documentary testimony, translations of the original papers of the inventor, and contemporary publications. By SYLVANUS P. THOMPSON, B.A., D.Sc., professor of experimental physics in University college, Bristol. London, E. & F. N. Spon, 1883. 9+182 p., 3 pl. 16°.

THE rapid development of the literature of the telephone, and the wide-spread interest in matters relating to it, have rendered the most important details of its history familiar to the general reading public, as well as to the scientific world. The account of the life and labors of Philipp Reis, by Prof. S. P. Thompson, while repeating many of these well-known details, contains some interesting notices of the life and personal characteristics of the inventor, and of the various steps by which he brought his instruments to their final stage. Following the brief biographical sketch, are descriptions of the various forms of apparatus devised by Reis, with numerous illustrations; a statement of what the author terms the inventor's claim; copies of Reis's own publications respecting his invention, and of certain contemporary accounts of it and its operation; with the testimony of persons who witnessed his experiments. An appendix discusses the variable resistance of imperfect contacts, a comparison of Reis's receiver with later instruments, the doctrine of undulatory currents, with some additional notes and references relating to Reis's invention.

Had the efforts of the author been directed to the presentation of these things as matters of history merely, the book might be regarded as a valuable and interesting summary of facts relating to an important invention, and would demand but a brief notice here; but a cursory examination of it is sufficient to show that the author has failed to maintain that judicial attitude of mind which is indispensable to the just

and impartial record of historic verities. His book is throughout a labored special pleading, with the attempt to prove that Reis's invention not only anticipated, but actually embodied, the essential features of the present telephonic apparatus. Space will not permit the consideration of all the points which might be criticised, nor is it necessary. A few of the most important are sufficient to illustrate the spirit which pervades the work, and to show how the facts of history are perverted in the endeavor to maintain a false and illogical position.

It has been generally accepted as true, that Reis designed his transmitter to act as a contact-breaker, which should open and close the circuit once for each vibration produced by the sound to be transmitted. The support for this view is found not only in the repeated statements of Reis himself, but also in the construction of the apparatus. Reis says, in his own description of his transmitter (p. 56), "each sound-wave effects an opening and a closing of the current;" and again, in his letter to Mr. Ladd (p. 84), "If a person sing at the station A, in the tube (x) the vibrations of air will pass into the box and move the membrane above; thereby the platina foot (c) of the movable angle will be lifted up and will thus open the stream at every condensation of air in the box. The stream will be re-established at every rarefaction. For this manner the steel axis at station B will be magnetic once for every full vibration."

With these and other most distinct statements of Reis, as to the intention and action of his apparatus, before him, Professor Thompson, nevertheless, asserts that it was never designed to break the circuit. Thus, on p. 14 he says, "Theoretically, the last was no more perfect than the first, and they all embody the same fundamental idea: they only differ in the mechanical means of carrying out to a greater or less degree of perfection the one common principle of imitating the mechanism of the human ear, and applying that mechanism to affect or control a current of electricity by varying the degree of contact at a loose joint in the circuit." And again (p. 132), "Now this operation of varying the degree of pressure in order to vary the resistance of the interruptor or contact-regulator, was distinctly contemplated by Reis." Further, the author maintains that the combination of an adjusting-screw with a spring shows that Reis intended the platinum contact-piece to have a following motion, so as to make a contact with varying pressure. He says on p. 133, "By employing these following-springs, he introduced, in

fact the element of *elasticity* into his interruptor; and clearly he introduced it for the very purpose of avoiding abrupt breaking of the contact." If we examine the illustrations of the different forms of the transmitting apparatus, we shall see that this device was employed for a very different purpose. In the earliest form, represented in fig. 1, the screw presses the spring *away* from the membrane, and, when the latter recedes in its vibration, the spring carrying the platinum point is prevented by the screw from following it, — an arrangement that tends to prevent, and was designed to prevent,

a following contact, and insures a breaking of the circuit when the distance of the point is properly adjusted. The same is the case with the form of transmitter illustrated in fig. 2. In the form represented in fig. 3, the screw is present, and works against a spring; but the screw passes through a stout and firm piece of metal, and presses the spring which carries the contact-piece forward, that is, toward the membrane, thus giving it a rigid support. The spring serves merely to push back the con-

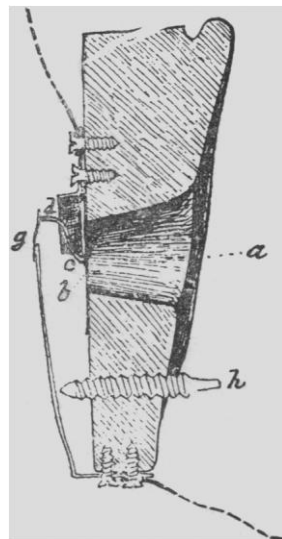


FIG. 1.

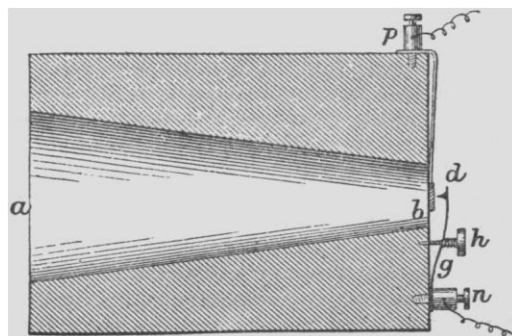


FIG. 2.

tact-piece when the screw is reversed, a very simple and common mechanical device for giving motion in opposition to the thrust of a screw. In the lever form, seen in figs. 4 and 5, the

screw is arranged in a similar manner, so as to regulate the distance of the contact-piece from the end of the lever most remote from the membrane. In all these instruments the screw acting upon the spring is expressly contrived to facilitate such an adjustment as will insure

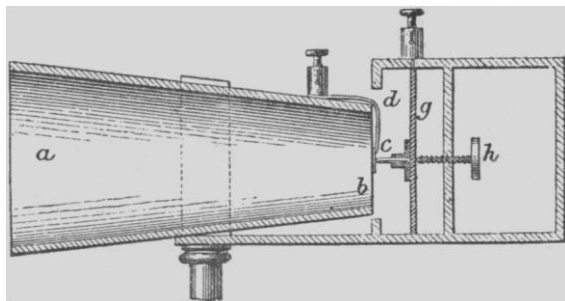


FIG. 3.

the breaking of contact under the impact of the sound-waves. Its function is related to the tension and elasticity of the membrane, to make a pressure so light in any case, that the vibrations should be able, without fail, to separate the contact-pieces.

To say, as Professor Thompson repeatedly does, that Reis employed his mechanism with the express intention of producing a variable current by the change of contact-resistance, and that he consciously and purposely utilized this principle, — at that time hardly recognized anywhere, and of which the practical application was not discovered till several years later, — is a gross misrepresentation, and an utter perversion of the facts. Reis did not know, and could not know, that the strength of a current could be controlled by the varying pressure of the conducting-surfaces between which it passes. Nowhere in his writings, — whether in his description of the instruments, or in the prospectus issued with them, or in his letter to Mr. Ladd, — nor in the article of Professor Böttger and the report of Von Legat, is there the remotest suggestion that the transmitter acted, or was intended to act, otherwise than by breaking the circuit. Nor is any thing of the kind to be found in any of the publications of the day, relating to this matter. With the knowledge which we now possess, of the varying resistance of pressure-contacts, it is indeed easy, by a slight modification, to cause the contact-pieces to vary the current by change of pressure, and thus reproduce the vibration-form with approximate accuracy. But to do this, it is necessary to prevent them from separating so as to break contact and interrupt the current. Such a modification, however slight it may be,

totally changes the function of the contact-pieces, and amounts to a radical transformation of the apparatus. It is the very thing Reis studiously sought to prevent.

That Reis speaks of the form of acoustical vibrations, and their graphical representation by a curve, is no proof that he supposed his transmitter to act otherwise than by breaking the circuit. Yet Professor Thompson says (p. 165), "It is certain that Reis did not in any of his writings explicitly name an undulatory current: but it is equally certain that, whether he mentioned it or not, he both used one and intended to use one." Reis nowhere claims that his apparatus realized the normal vibration-form, even in the case of a simple tone; and there is no evidence in all his writings to show that he had ever considered the motions at the receiver to be the same as those of the original sound, except so far as there was a correspondence in period or rate of these motions with those at the transmitter. The idea of causing the motions in the receiver to have the same vibration-form as those in the transmitter originated with Bell, as did the method of securing this correspondence, which is indispensable to the reproduction of spoken words, by the use of an undulatory current. Says Sir William Thomson (*Tel. journ. and electr. rev.*, v. 293), "Mr. Graham Bell conceived the idea — the wholly novel and original idea — of giving continuity to the shocks, and of producing currents which would be in simple proportion to the motion of the air pro-

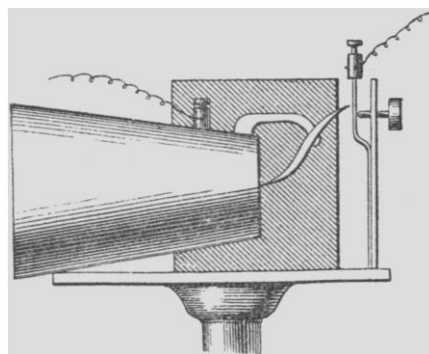


FIG. 4.

duced by the voice, and of reproducing that effect at the remote end of a telegraphic wire." The author of this book will scarcely have the hardihood to assert that his illustrious countryman, one of the greatest masters in electrical science, uttered these words in ignorance of a thing so well known as Reis's telephone.

As a further support to his position, the author lays great stress upon the statement that Reis's apparatus could and did transmit spoken words so as to be understood. As to the fact of speech having been transmitted occasionally, it is doubtless true that some words were recognized, but imperfectly, and with difficulty; and it is true, also, that when imperfectly meeting the conditions set upon it by the inventor, the apparatus, when applied to transmit spoken words, will, with skilful handling, sometimes 'deviate into sense' so far, that an occasional word or short phrase can be made out with effort, by attentive listening with the ear close to it. Professor Böttger, who took an enthusiastic interest in the matter, says that the operators could communicate words with each other, but adds, 'only such, however, as they had already heard frequently.' Of the other experimenters and witnesses whose testimony is given in the book, some were able to understand portions of what was said; others failed. Every one familiar with telephonic experiments knows how easy it is to recognize these familiar phrases by the mere intonation, and how different this is from understanding words not previously known. Is it any thing surprising, that the words of a familiar song should appear to be recognized when the air is heard? Granted that the spoken words were sometimes reproduced so as to be understood, it must also be admitted that the apparatus accomplished this so imperfectly as to be of no practical value. To make it practically efficient required a modification that was in itself a radical change and a distinct invention. That this was also Reis's opinion, will be seen from the extracts given in a subsequent paragraph.

There is good evidence, in the later writings and advertisements of Reis, that he had come to the conclusion that the faithful reproduction of the complex motions which occur in articulate speech was impossible, and that he had silently abandoned the idea of reproducing

speech. A further proof of this is found in the addition of the telegraphic signal-apparatus to the later forms of the instrument, to enable the experimenters to communicate with each other. Professor Thompson's argument that the Morse signal-apparatus, if intended for verbal communication, should have been reversed, meets the facts but half way; for the complete telephonic installation required a transmitter and a receiver at each end of the wire, so that the Morse signals could be sent in either direction with the same facility as the telephonic. Moreover, as if to prevent any possible question as to its use, Reis himself expressly says that the Morse apparatus is for the purpose of enabling the operators to com-

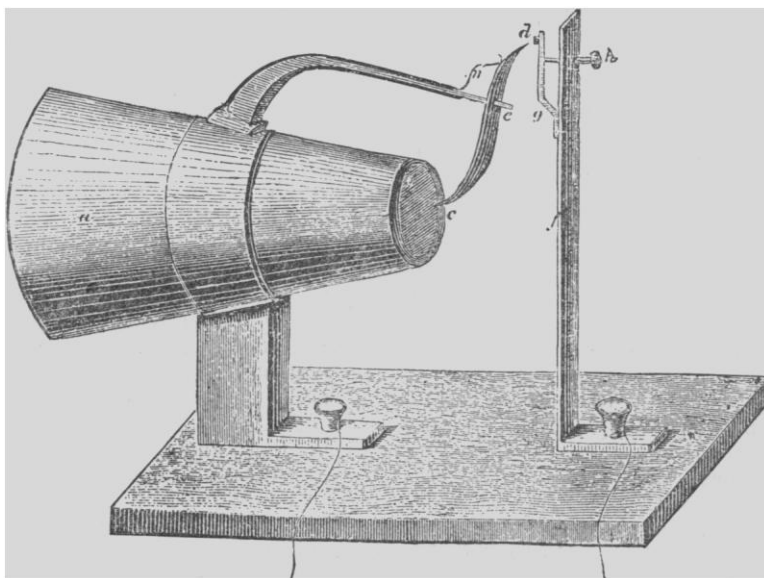


FIG. 5.

municate with each other; and, in the prospectus issued with the instruments, he describes a special alphabet, which he had devised to enable words to be spelled out. If these could be transmitted telephonically, why take this unnecessary trouble? This very provision is a most emphatic testimony that Reis, at this time, had become convinced that the apparatus as a transmitter of speech was a failure, and that, his original idea having proved impracticable, he had contented himself with sending musical tones.

In respect to this point, the letter of Reis, written by himself in English to Mr. Ladd, and given at p. 81, is most significant. He says, "Tunes and sounds of any kind are only brought to our conception by the condensations

and rarefactions of air or any other medium in which we may find ourselves." And again, on p. 82, "these were the principles which (*sic*) guided me in my invention. They were sufficient to induce me to try the reproduction of tunes at any distance." And again, on the same page, "The apparatus consists of two separated parts; one for the singing-station A, and the other for the hearing-station B." Also in the same letter, p. 84, "If a person sing at the station A, in the tube (*x*) the vibrations will pass into the box and move the membrane above." Respecting the word 'tunes,' used by Reis, the author remarks, in a foot-note to p. 81, "This word, as the context and ending of the paragraph shows, should have been written *tones*. The letter, written in English by Reis himself, is wonderfully free from inaccuracies of composition; the slip here noted being a most pardonable one since the plural of the German *ton* is *tönen*, the very pronunciation of which would account for the confusion in the mind of one unaccustomed to write in English." The resemblance of *tönen* to *tunes* is not so remarkably close that it would be likely to mislead one whose knowledge of English is such as Reis shows himself in this letter to possess. The author does not attempt the explanation of the words 'singing' and 'sing' in the same letter. It is surprising that he should have allowed these words to pass unnoticed, for it was vital to his argument to prove that Reis mistook them for 'speaking' and 'speak.' The resemblance is about as close as in the other case, but in neither is the explanation likely to be admitted by the unprejudiced reader.

In taking himself back to the time of Reis's telephone, the author has failed to identify himself with the conditions of that time, and to leave behind him the subsequent acquisitions of science. He makes statements and claims which could only find their justification in a world very differently furnished with facts from this one. As an illustration of the mental disposition resulting from this, the following sentence from the author's preface may serve: "The testimony now adduced as to the aim of Philipp Reis's invention, and the measure of success which he himself attained, is such, in the author's opinion, and in the opinion, he trusts, of all right-thinking persons, to place beyond cavil the rightfulness of the claim which Reis himself put forward of being the inventor of the Telephone." But did any one ever dispute this claim during his life? and has the author forgotten that no possible basis for a rival claim existed until more than two years

after Reis's death?—unless we except the suggestions of Bourseul, in 1854, which, while they certainly did anticipate the general idea of Reis's invention, were never carried to the stage of experiment, and were never set up in opposition to him, unless it has been done recently. The author can hardly have been ignorant of these suggestions; but, if not, he has carefully refrained from mentioning them. Reis never claimed that no new principle could ever be discovered which would enable the ends he sought to be attained in a different way, and more perfectly. His first article upon the subject ends with these words: "There may probably remain much more yet to be done for the utilization of the telephone in practice. For physics, however, it has already sufficient interest in that it has opened out a new field of labor." And Von Legat closes his report with this remark: "There remains no doubt, that, before expecting a practical utilization with serviceable results, that which has been spoken of will require still considerable improvement, and, in particular, mechanical science must complete the apparatus to be used."

The chief aim of the book is clearly this, — to endeavor, in direct opposition to the facts, to establish the untenable proposition that the Reis transmitter was designedly contrived by him to vary the contact-resistance by pressure, giving it a microphonic action, failure to accomplish which is fatal to its success in conveying spoken words. Professor Thompson has not always been of this opinion, and in another place he has given a correct account of the relation of Reis's invention to that of Bell. In his 'Elementary lessons in electricity and magnetism,' published in 1881, we find, on pp. 405 and 406, these words, — "The first attempt to transmit sounds electrically was made in 1852 [misprint for 1862] by Reis, who succeeded in conveying musical tones by an imperfect telephone. The transmitting part of Reis's telephone consisted of a battery and a contact-breaker, the latter being formed of a stretched membrane, capable of taking up sonorous vibrations, and having attached to it a thin strip of platinum, which, as it vibrated, beat to and fro against the tip of a platinum wire, so making and breaking contact. . . . Reis also transmitted speech with this instrument, but very imperfectly, for the tones of speech cannot be transmitted by abrupt interruptions of the current. . . . In 1876 Graham Bell invented the articulating telephone. In this instrument the speaker talks to an elastic disk of thin sheet-iron, which vibrates, and transmits its every movement electrically to a

similar disk in a similar telephone at a distant station, causing it to vibrate in an identical manner, and therefore to emit identical sounds." Here we have Reis spoken of as inventing 'an imperfect telephone,' while Bell invented 'the articulating telephone.' Reis's instrument was a 'contact-breaker,' and conveyed 'musical tones.' Reis's instrument transmitted speech 'very imperfectly,' and there is not the slightest suggestion of microphonic action in the transmitter. Yet two years later we have statements diametrically opposed to these.

The least that can be said of such varying and contradictory evidence is, that it totally destroys the credibility of the witness, and nullifies his claim to be accepted as a scientific authority, unless good reason is shown for the different opinion. The documents quoted in the book give no substantial reason for this change of ground, as they add very little of any importance to what was already generally known. The motive for the later opinions may be more intelligibly traced in the following items, which will be found in the *Telegraphic journal and electrical review*, vol. xii. p. 72, Jan., 1883, and p. 317, April 14, 1883, in the list of English patents:—"2578. Telephonic instruments. SYLVANUS P. THOMPSON. Dated May 31. 6d. This invention relates to telephonic instruments, and chiefly to improvements in receivers of a well-known form or type, invented by Phillip Reis." "3803. Improvements in telephonic apparatus. SYLVANUS P. THOMPSON. Dated August 9. 6d. Relates to telephonic transmitters based upon the principle discovered by Philipp Reis in 1861, namely that of employing current-regulators actuated, either directly or indirectly, by the sound-waves produced by the voice. By the term 'current-regulator,' the inventor means a device similar to that employed by Reis, wherein a loose contact between two parts of a circuit (in which are included a battery and a telephonic receiver) offers greater or less resistance to the flow of the electric current, the degree of intimacy of contact between the conducting-pieces being altered by the vibrations of the voice."

For a contrast of colors, we may put side by side with these sentences the following, from the preface to the book now under consideration: "To set forth the history of this long-neglected inventor and of his instrument, and to establish upon its own merits, without special pleading, and without partiality, the nature of that much-misunderstood and much-abused invention, has been the aim of the writer. . . . He has nothing to gain by making Reis's in-

vention appear either better or worse than it really was."

Further comment upon the value of such testimony as is contained in this book is superfluous. What Reis accomplished, and what he failed to do, are now familiar matters of history. His well-earned fame can only suffer from such misstatement of facts, and the unjust exaggeration of his actual achievements.

OBLIGATIONS OF MATHEMATICS TO PHILOSOPHY, AND TO QUESTIONS OF COMMON LIFE.¹—I.

SINCE our last meeting, we have been deprived of three of our most distinguished members. The loss by the death of Professor Henry John Stephen Smith is a very grievous one to those who knew and admired and loved him, to his university, and to mathematical science, which he cultivated with such ardor and success. I need hardly recall that the branch of mathematics to which he had specially devoted himself was that most interesting and difficult one, the theory of numbers. The immense range of this subject, connected with and ramifying into so many others, is nowhere so well seen as in the series of reports on the progress thereof, brought up, unfortunately, only to the year 1865, contributed by him to the reports of the association; but it will still better appear, when to these are united (as will be done in the collected works in course of publication by the Clarendon Press) his other mathematical writings, many of them containing his own further developments of theories referred to in the reports. There have been recently or are being published many such collected editions, — Abel, Cauchy, Clifford, Gauss, Green, Jacobi, Lagrange, Maxwell, Riemann, Steiner. Among these, the works of Henry Smith will occupy a worthy position.

More recently, Gen. Sir Edward Sabine, K.C.B., for twenty-one years general secretary of the association, and a trustee, president of the meeting at Belfast in the year 1852, and for many years treasurer, and afterwards president of the Royal society, has been taken from us at an age exceeding the ordinary age of man. Born October, 1788, he entered the Royal artillery in 1803, and commanded batteries at the siege of Fort Erie in 1814; made magnetic and other observations in Ross and Parry's north-polar exploration in 1818-19, and in a series of other voyages. He contributed to the association reports on magnetic forces in 1836, 1837, and 1838, and about forty papers to the *Philosophical transactions*; originated the system of magnetic observatories, and otherwise signally promoted the science of terrestrial magnetism.

There is yet a very great loss, — another late presi-

¹ Inaugural address by ARTHUR CAYLEY, M.A., D.C.L. LL.D., F.R.S., Sadlerian professor of pure mathematics in the University of Cambridge, president of the British association for the advancement of science, for the Southport meeting. From advance proofs kindly furnished by the editors of *Nature*.

dent and trustee of the association; one who has done for it so much, and has so often attended the meetings; whose presence among us at this meeting we might have hoped for, — the president of the Royal society, William Spottiswoode. It is unnecessary to say any thing of his various merits. The place of his burial, the crowd of sorrowing friends who were present in the Abbey, bear witness to the esteem in which he was held.

I take the opportunity of mentioning the completion of a work promoted by the association, — the determination, by Mr. James Glaisher, of the least factors of the missing three out of the first nine million numbers. The volume containing the sixth million is now published.

I wish to speak to you to-night upon mathematics. I am quite aware of the difficulty arising from the abstract nature of my subject; and if, as I fear, many or some of you, recalling the presidential addresses at former meetings, — for instance, the *résumé* and survey which we had at York of the progress, during the half-century of the lifetime of the association, of a whole circle of sciences (biology, paleontology, geology, astronomy, chemistry) so much more familiar to you, and in which there was so much to tell of the fairy-tales of science; or, at Southampton, the discourse of my friend, who has in such kind terms introduced me to you, on the wondrous practical applications of science to electric lighting, telegraphy, the St. Gothard Tunnel and the Suez Canal, gun-cotton, and a host of other purposes, and with the grand concluding speculation on the conservation of solar energy: — if, I say, recalling these or any earlier addresses, you should wish that you were now about to have, from a different president, a discourse on a different subject, I can very well sympathize with you in the feeling.

But, be this as it may, I think it is more respectful to you that I should speak to you upon, and do my best to interest you in, the subject which has occupied me, and in which I am myself most interested. And, in another point of view, I think it is right that the address of a president should be on his own subject, and that different subjects should be thus brought in turn before the meetings. So much the worse, it may be, for a particular meeting; but the meeting is the individual, which, on evolution principles, must be sacrificed for the development of the race.

Mathematics connect themselves, on the one side, with common life and the physical sciences; on the other side, with philosophy in regard to our notions of space and time, and in the questions which have arisen as to the universality and necessity of the truths of mathematics, and the foundation of our knowledge of them. I would remark here, that the connection (if it exists) of arithmetic and algebra with the notion of time is far less obvious than that of geometry with the notion of space.

As to the former side: I am not making before you a defence of mathematics; but, if I were, I should desire to do it in such manner as in the 'Republic' Socrates was required to defend justice, — quite irre-

spectively of the worldly advantages which may accompany a life of virtue and justice, — and to show, that, independently of all these, justice was a thing desirable in itself and for its own sake, *not* by speaking to you of the utility of mathematics in any of the questions of common life or of physical science. Still less would I speak of this utility before, I trust, a friendly audience, interested or willing to appreciate an interest in mathematics in itself and for its own sake. I would, on the contrary, rather consider the obligations of mathematics to these different subjects as the sources of mathematical theories, now as remote from them, and in as different a region of thought, — for instance, geometry from the measurement of land, or the theory of numbers from arithmetic, — as a river at its mouth is from its mountain source.

On the other side: the general opinion has been, and is, that it is indeed by experience that we arrive at the truths of mathematics, but that experience is not their proper foundation. The mind itself contributes something. This is involved in the Platonic theory of reminiscence. Looking at two things — trees or stones or any thing else — which seem to us more or less equal, we arrive at the idea of equality; but we must have had this idea of equality before the time when, first seeing the two things, we were led to regard them as coming up more or less perfectly to this idea of equality; and the like as regards our idea of the beautiful, and in other cases.

The same view is expressed in the answer of Leibnitz, the '*nisi intellectus ipse*,' to the scholastic dictum, '*Nihil in intellectu quod non prius in sensu*' ('There is nothing in the intellect which was not first in sensation' — 'except [said Leibnitz] the intellect itself'). And so again, in the 'Critick of pure reason,' Kant's view is, that while there is no doubt but that all our cognition begins with experience, we are nevertheless in possession of cognitions *a priori*, independent, not of this or that experience, but absolutely so of all experience, and in particular that the axioms of mathematics furnish an example of such cognitions *a priori*. Kant holds, further, that space is no empirical conception which has been derived from external experiences, but that, in order that sensations may be referred to something external, the representation of space must already lie at the foundation, and that the external experience is itself first only possible by this representation of space. And, in like manner, time is no empirical conception which can be deduced from an experience, but it is a necessary representation lying at the foundation of all intuitions.

And so in regard to mathematics, Sir W. R. Hamilton, in an introductory lecture on astronomy (1836), observes, "These purely mathematical sciences of algebra and geometry are sciences of the pure reason, deriving no weight and no assistance from experiment, and isolated, or at least isolable, from all outward and accidental phenomena. The idea of order, with its subordinate ideas of number and figure, we must not, indeed, call innate ideas, if that phrase be defined to imply that all men must possess them with

equal clearness and fulness: they are, however, ideas which seem to be so far born with us that the possession of them in any conceivable degree is only the development of our original powers, the unfolding of our proper humanity."

The general question of the ideas of space and time, the axioms and definitions of geometry, the axioms relating to number, and the nature of mathematical reasoning, are fully and ably discussed in Whewell's "Philosophy of the inductive sciences" (1840), which may be regarded as containing an exposition of the whole theory.

But it is maintained by John Stuart Mill that the truths of mathematics, in particular those of geometry, rest on experience; and, as regards geometry, the same view is on very different grounds maintained by the mathematician Riemann.

It is not so easy as at first sight it appears, to make out how far the views taken by Mill in his 'System of logic ratiocinative and inductive' (ninth edition, 1879) are absolutely contradictory to those which have been spoken of. They profess to be so. There are most definite assertions (supported by argument): for instance, p. 263, "It remains to inquire what is the ground of our belief in axioms, what is the evidence on which they rest. I answer, they are experimental truths, generalizations from experience. The proposition 'Two straight lines cannot enclose a space,' or, in other words, two straight lines which have once met cannot meet again, is an induction from the evidence of our senses." But I cannot help considering a previous argument (p. 259) as very materially modifying this absolute contradiction. After inquiring, "Why are mathematics by almost all philosophers . . . considered to be independent of the evidence of experience and observation, and characterized as systems of necessary truth?" Mill proceeds (I quote the whole passage) as follows: "The answer I conceive to be, that this character of necessity ascribed to the truths of mathematics, and even (with some reservations to be hereafter made) the peculiar certainty ascribed to them, is a delusion, in order to sustain which it is necessary to suppose that those truths relate to and express the properties of purely imaginary objects. It is acknowledged that the conclusions of geometry are derived, partly at least, from the so-called definitions, and that these definitions are assumed to be correct representations, as far as they go, of the objects with which geometry is conversant. Now, we have pointed out, that, from a definition as such, no proposition, unless it be one concerning the meaning of a word, can ever follow, and that what apparently follows from a definition follows in reality from an implied assumption that there exists a real thing conformable thereto. This assumption, in the case of the definitions of geometry, is not strictly true. There exist no real things exactly conformable to the definitions. There exist no real points without magnitude, no lines without breadth, nor perfectly straight, no circles with all their radii exactly equal, nor squares with all their angles perfectly right. It will be said that the assumption does not extend to the actual, but only to the possible, ex-

istence of such things. I answer, that, according to every test we have of possibility, they are not even possible. Their existence, so far as we can form any judgment, would seem to be inconsistent with the physical constitution of our planet at least, if not of the universal [*sic*]. To get rid of this difficulty, and at the same time to save the credit of the supposed system of necessary truths, it is customary to say that the points, lines, circles, and squares which are the subjects of geometry exist in our conceptions merely, and are parts of our minds; which minds, by working on their own materials, construct an *a priori* science, the evidence of which is purely mental, and has nothing to do with outward experience. By howsoever high authority this doctrine has been sanctioned, it appears to me psychologically incorrect. The points, lines, and squares which any one has in his mind are (as I apprehend) simply copies of the points, lines, and squares, which he has known in his experience. Our idea of a point I apprehend to be simply our idea of the *minimum visibile*, the small portion of surface which we can see. We can reason about a line as if it had no breadth, because we have a power which we can exercise over the operations of our minds, — the power, when a perception is present to our senses, or a conception to our intellects, of *attending* to a part only of that perception or conception, instead of the whole. But we cannot *conceive* a line without breadth; we can form no mental picture of such a line: all the lines which we have in our mind are lines possessing breadth. If any one doubt this, we may refer him to his own experience. I much question if any one who fancies that he can conceive of a mathematical line thinks so from the evidence of his own consciousness. I suspect it is rather because he supposes, that, unless such a perception be possible, mathematics could not exist as a science, — a supposition which there will be no difficulty in showing to be groundless."

I think it may be at once conceded that the truths of geometry are truths precisely because they relate to and express the properties of what Mill calls 'purely imaginary objects.' That these objects do not exist in Mill's sense, that they do not exist in nature, may also be granted. That they are 'not even possible,' if this means not possible in an existing nature, may also be granted. That we cannot 'conceive' them depends on the meaning which we attach to the word 'conceive.' I would myself say that the purely imaginary objects are the only realities, the *ὄντως ὄντα*, in regard to which the corresponding physical objects are as the shadows in the cave; and it is only by means of them that we are able to deny the existence of a corresponding physical object. If there is no conception of straightness, then it is meaningless to deny the existence of a perfectly straight line.

But, at any rate, the objects of geometrical truth are the so-called imaginary objects of Mill; and the truths of geometry are only true, and *a fortiori* are only necessarily true, in regard to these so-called imaginary objects. And these objects, points, lines, circles, etc., in the mathematical sense of the terms,

have a likeness to, and are represented more or less imperfectly, — and, from a geometer's point of view, no matter how imperfectly, — by corresponding physical points, lines, circles, etc. I shall have to return to geometry, and will then speak of Riemann; but I will first refer to another passage of the 'Logic.'

Speaking of the truths of arithmetic, Mill says (p. 297) that even here there is one hypothetical element: "In all propositions concerning numbers, a condition is implied without which none of them would be true; and that condition is an assumption which may be false. The condition is, that $1=1$; that all the numbers are numbers of the same or of equal units." Here, at least, the assumption may be absolutely true: one shilling=one shilling in purchasing-power, although they may not be absolutely of the same weight and fineness. But it is hardly necessary: one coin+one coin=two coins, even if the one be a shilling and the other a half-crown. In fact, whatever difficulty be raisable as to geometry, it seems to me that no similar difficulty applies to arithmetic. Mathematician or not, we have each of us, in its most abstract form, the idea of a number. We can each of us appreciate the truth of a proposition in regard to numbers; and we cannot but see that a truth in regard to numbers is something different in kind from an experimental truth generalized from experience. Compare, for instance, the proposition that the sun, having already risen so many times, will rise to-morrow, and the next day, and the day after that, and so on, and the proposition that even and odd numbers succeed each other alternately *ad infinitum*: the latter, at least, seems to have the characters of universality and necessity. Or, again, suppose a proposition observed to hold good for a long series of numbers, — one thousand numbers, two thousand numbers, as the case may be: this is not only no proof, but it is absolutely no evidence, that the proposition is a true proposition, holding good for all numbers whatever. There are, in the theory of numbers, very remarkable instances of propositions observed to hold good for very long series of numbers, and which are nevertheless untrue.

I pass in review certain mathematical theories.

In arithmetic and algebra, or, say, in analysis, the numbers or magnitudes which we represent by symbols are, in the first instance, ordinary (that is, positive) numbers or magnitudes. We have also in analysis, and in analytical geometry, *negative* magnitudes. There has been, in regard to these, plenty of philosophical discussion, and I might refer to Kant's paper, 'Ueber die negativen gröszen in die weltweisheit' (1763); but the notion of a negative magnitude has become quite a familiar one, and has extended itself into common phraseology. I may remark that it is used in a very refined manner in book-keeping by double entry.

But it is far otherwise with the notion which is really the fundamental one (and I cannot too strongly emphasize the assertion), underlying and pervading the whole of modern analysis and geometry, — that of imaginary magnitude in analysis, and of imaginary space (or space as a *locus in quo* of imaginary

points and figures) in geometry. I use in each case the word 'imaginary' as including real. This has not been, so far as I am aware, a subject of philosophical discussion or inquiry. As regards the older metaphysical writers, this would be quite accounted for by saying that they knew nothing, and were not bound to know any thing, about it. But at present, and considering the prominent position which the notion occupies, — say, even, that the conclusion were that the notion belongs to mere technical mathematics, or has reference to nonentities in regard to which no science is possible, — still it seems to me, that, as a subject of philosophical discussion, the notion ought not to be thus ignored. It should at least be shown that there is a right to ignore it.

Although in logical order I should perhaps now speak of the notion just referred to, it will be convenient to speak first of some other quasi-geometrical notions, — those of more-than-three-dimensional space, and of non-Euclidian two- and three-dimensional space, and also of the generalized notion of distance. It is in connection with these, that Riemann considered that our notion of space is founded on experience, or, rather, that it is only by experience that we know that our space is Euclidian space.

It is well known that Euclid's twelfth axiom, even in Playfair's form of it, has been considered as needing demonstration, and that Lobatschewsky constructed a perfectly consistent theory, wherein this axiom was assumed not to hold good, or, say, a system of non-Euclidian plane geometry. There is a like system of non-Euclidian solid geometry. My own view is, that Euclid's twelfth axiom, in Playfair's form of it, does not need demonstration, but is part of our notion of space, of the physical space of our experience, — the space, that is, with which we become acquainted by experience, but which is the representation lying at the foundation of all external experience. Riemann's view, before referred to, may, I think, be said to be, that, having *in intellectu* a more general notion of space (in fact, a notion of non-Euclidian space), we learn by experience that space (the physical space of our experience) is — if not exactly, at least to the highest degree of approximation — Euclidian space.

But suppose the physical space of our experience to be thus only approximately Euclidian space: what is the consequence which follows? *Not* that the propositions of geometry are only approximately true, but that they remain absolutely true in regard to that Euclidian space which has been so long regarded as being the physical space of our experience.

It is interesting to consider two different ways in which, without any modification at all of our notion of space, we can arrive at a system of non-Euclidian (plane or two-dimensional) geometry; and the doing so will, I think, throw some light on the whole question.

First, imagine the earth a perfectly smooth sphere; understand by a plane the surface of the earth, and, by a line, the apparently straight line (in fact, an arc of great circle) drawn on the surface. What experience would in the first instance teach would be Eu-

clidian geometry: there would be intersecting lines, which, produced a few miles or so, would seem to go on diverging, and apparently parallel lines, which would exhibit no tendency to approach each other; and the inhabitants might very well conceive that they had by experience established the axiom that two straight lines cannot enclose a space, and the axiom as to parallel lines. A more extended experience and more accurate measurements would teach them that the axioms were each of them false; and that any two lines, if produced far enough each way, would meet in two points: they would, in fact, arrive at a spherical geometry, accurately representing the properties of the two-dimensional space of their experience. But their original Euclidian geometry would not the less be a true system; only it would apply to an ideal space, not the space of their experience.

Secondly, consider an ordinary, indefinitely extended plane; and let us modify only the notion of distance. We measure distance, say, by a yard measure or a foot rule, any thing which is short enough to make the fractions of it of no consequence (in mathematical language, by an infinitesimal element of length). Imagine, then, the length of this rule constantly changing (as it might do by an alteration of temperature), but under the condition that its actual length shall depend only on its situation on the plane, and on its direction; viz., if for a given situation and direction it has a certain length, then whenever it comes back to the same situation and direction it must have the same length. The distance along a given straight or curved line between any two points could then be measured in the ordinary manner with this rule, and would have a perfectly determinate value; it could be measured over and over again, and would always be the same: but of course it would be the distance, not in the ordinary acceptance of the term, but in quite a different acceptance. Or in a somewhat different way: if the rate of progress from a given point in a given direction be conceived as depending only on the configuration of the ground, and the distance along a given path between any two points thereof be measured by the time required for traversing it, then in this way, also, the distance would have a perfectly determinate value; but it would be a distance, not in the ordinary acceptance of the term, but in quite a different acceptance; and, corresponding to the new notion of distance, we should have a new non-Euclidian system of plane geometry. All theorems involving the notion of distance would be altered.

We may proceed farther. Suppose that as the rule moves away from a fixed central point of the plane it becomes shorter and shorter: if this shortening take place with sufficient rapidity, it may very well be that a distance which in the ordinary sense of the word is finite will in the new sense be infinite. No number of repetitions of the length of the ever-shortening rule will be sufficient to cover it. There will be surrounding the central point a certain finite area, such that (in the new acceptance of the term 'distance') each point of the boundary thereof will be at an infinite

distance from the central point. The points outside this area you cannot by any means arrive at with your rule: they will form a *terra incognita*, or, rather, an unknowable land (in mathematical language, an imaginary or impossible space); and the plane space of the theory will be that within the finite area, that is, it will be finite instead of infinite.

We thus, with a proper law of shortening, arrive at a system of non-Euclidian geometry which is essentially that of Lobatschewsky; but, in so obtaining it, we put out of sight its relation to spherical geometry. The three geometries (spherical, Euclidian, and Lobatschewsky's) should be regarded as members of a system: viz., they are the geometries of a plane (two-dimensional) space of constant positive curvature, zero curvature, and constant negative curvature, respectively; or, again, they are the plane geometries corresponding to three different notions of distance. In this point of view, they are Klein's elliptic, parabolic, and hyperbolic geometries respectively.

Next as regards solid geometry: we can, by a modification of the notion of distance (such as has just been explained in regard to Lobatschewsky's system), pass from our present system to a non-Euclidian system. For the other mode of passing to a non-Euclidian system, it would be necessary to regard our space as a flat three-dimensional space existing in a space of four dimensions (i.e., as the analogue of a plane existing in ordinary space), and to substitute for such flat three-dimensional space a curved three-dimensional space, say, of constant positive or negative curvature. In regarding the physical space of our experience as possibly non-Euclidian, Riemann's idea seems to be that of modifying the notion of distance, not that of treating it as a locus in four-dimensional space.

I have just come to speak of four-dimensional space. What meaning do we attach to it? or can we attach to it any meaning? It may be at once admitted that we cannot conceive of a fourth dimension of space; that space as we conceive of it, and the physical space of our experience, are alike three-dimensional. But we can, I think, conceive of space as being two- or even one-dimensional; we can imagine rational beings living in a one-dimensional space (a line) or in a two-dimensional space (a surface), and conceiving of space accordingly, and to whom, therefore, a two-dimensional space or (as the case may be) a three-dimensional space would be as inconceivable as a four-dimensional space is to us. And very curious speculative questions arise. Suppose the one-dimensional space a right line, and that it afterwards becomes a curved line: would there be any indication of the change? or, if originally a curved line, would there be any thing to suggest to them that it was not a right line? Probably not; for a one-dimensional geometry hardly exists. But let the space be two-dimensional, and imagine it originally a plane, and afterwards bent (converted, that is, into some form of developable surface), or converted into a curved surface; or imagine it originally a developable or curved surface. In the former case there should be an indication of the change, for the

geometry originally applicable to the space of their experience (our own Euclidian geometry) would cease to be applicable; but the change could not be apprehended by them as a bending or deformation of the plane, for this would imply the notion of a three-dimensional space in which this bending or deformation could take place. In the latter case their geometry would be that appropriate to the developable or curved surface which is their space; viz., this would be their Euclidian geometry. Would they ever have arrived at our own more simple system? But take the case where the two-dimensional space is a plane, and imagine the beings of such a space familiar with our own Euclidian plane geometry: if, a third dimension being still inconceivable by them, they were by their geometry or otherwise led to the notion of it, there would be nothing to prevent them from forming a science such as our own science of three-dimensional geometry.

Evidently, all the foregoing questions present themselves in regard to ourselves, and to three-dimensional space as we conceive of it, and as the physical space of our experience. And I need hardly say that the first step is the difficulty, and that, granting a fourth dimension, we may assume as many more dimensions as we please. But, whatever answer be given to them, we have, as a branch of mathematics, potentially if not actually, an analytical geometry of n -dimensional space. I shall have to speak again upon this.

Coming now to the fundamental notion already referred to, — that of imaginary magnitude in analysis, and imaginary space in geometry; I connect this with two great discoveries in mathematics, made in the first half of the seventeenth century, — Harriot's representation of an equation in the form $f(x)=0$, and the consequent notion of the roots of an equation as derived from the linear factors of $f(x)$ (Harriot, 1560-1621: his 'Algebra,' published after his death, has the date 1631); and Descartes' method of co-ordinates, as given in the 'Géométrie' forming a short supplement to his 'Traité de la méthode,' etc. (Leyden, 1637).

I show how by these we are led analytically to the notion of imaginary points in geometry. For instance: we arrive at the theorem that a straight line and circle in the same plane intersect *always* in two points, real or imaginary. The conclusion as to the two points of intersection cannot be contradicted by experience. Take a sheet of paper and draw on it the straight line and circle, and try. But you might say, or at least be strongly tempted to say, that it is meaningless. The question, of course, arises, What is the meaning of an imaginary point? and, further, In what manner can the notion be arrived at geometrically?

There is a well-known construction in perspective for drawing lines through the intersection of two lines which are so nearly parallel as not to meet within the limits of the sheet of paper. You have two given lines which do not meet, and you draw a third line, which, when the lines are all of them produced, is found to pass through the intersection of the given

lines. If, instead of lines, we have two circular arcs not meeting each other, then we can, by means of these arcs, construct a line; and if, on completing the circles, it is found that the circles intersect each other in two real points, then it will be found that the line passes through these two points: if the circles appear not to intersect, then the line will appear not to intersect either of the circles. But the geometrical construction being in each case the same, we say that in the second case, also, the line passes through the two intersections of the circles.

Of course, it may be said in reply, that the conclusion is a very natural one, provided we assume the existence of imaginary points; and that, this assumption not being made, then, if the circles do not intersect, it is meaningless to assert that the line passes through their points of intersection. The difficulty is not got over by the analytical method before referred to, for this introduces difficulties of its own. Is there, in a plane, a point the co-ordinates of which have given imaginary values? As a matter of fact, we do consider, in plane geometry, imaginary points introduced into the theory analytically or geometrically, as above.

The like considerations apply to solid geometry; and we thus arrive at the notion of imaginary space as a *locus in quo* of imaginary points and figures.

I have used the word 'imaginary' rather than 'complex,' and I repeat that the word has been used as including real. But, this once understood, the word becomes in many cases superfluous, and the use of it would even be misleading. Thus: 'a problem has so many solutions.' This means so many imaginary (including real) solutions. But if it were said that the problem had 'so many imaginary solutions,' the word 'imaginary' would here be understood to be used in opposition to real. I give this explanation the better to point out how wide the application of the notion of the imaginary is; viz. (unless expressly or by implication excluded), it is a notion implied and presupposed in all the conclusions of modern analysis and geometry. It is, as I have said, the fundamental notion underlying and pervading the whole of these branches of mathematical science.

I consider the question of the geometrical representation of an imaginary variable. We represent the imaginary variable $x + iy$ by means of a point in a plane, the co-ordinates of which are (x, y) . This idea, due to Gauss, dates from about the year 1831. We thus picture to ourselves the succession of values of the imaginary variable $x + iy$ by means of the motion of the representative point: for instance, the succession of values corresponding to the motion of the point along a closed curve to its original position. The value $X + iY$ of the function can, of course, be represented by means of a point (taken for greater convenience in a different plane), the co-ordinates of which are X, Y .

We may consider, in general, two points, moving each in its own plane; so that the position of one of them determines the position of the other, and consequently the motion of the one determines the motion of the other. For instance: the two points may

be the tracing-point and the pencil of a pentagraph. You may with the first point draw any figure you please: there will be a corresponding figure drawn by the second point,—for a good pentagraph, a copy on a scale different, it may be; for a badly adjusted pentagraph, a distorted copy; but the one figure will always be a sort of copy of the first, so that to each point of the one figure there will correspond a point in the other figure.

In the case above referred to, where one point represents the value $x+iy$ of the imaginary variable, and the other the value $X+iY$ of some function, $\phi(x+iy)$, of that variable, there is a remarkable relation between the two figures: this is the relation of orthomorphic projection, the same which presents itself between a portion of the earth's surface and the representation thereof by a map on the stereographic projection or on Mercator's projection; viz., any indefinitely small area of the one figure is represented in the other figure by an indefinitely small area of the same shape. There will possibly be for different parts of the figure great variations of scale, but the shape will be unaltered. If for the one area the boundary is a circle, then for the other area the boundary will be a circle: if for one it is an equilateral triangle, then for the other it will be an equilateral triangle.

I have been speaking of an imaginary variable ($x+iy$), and of a function, $\phi(x+iy)=X+iY$, of that variable; but the theory may equally well be stated in regard to a plane curve: in fact, the $x+iy$ and the $X+iY$ are two imaginary variables connected by an equation. Say their values are u and v , connected by an equation, $F(u, v)=0$: then, regarding u, v , as the co-ordinates of a point *in plano*, this will be a point on the curve represented by the equation. The curve, in the widest sense of the expression, is the whole series of points, real or imaginary,

the co-ordinates of which satisfy the equation; and these are exhibited by the foregoing corresponding figures in two planes. But, in the ordinary sense, the curve is the series of real points, with co-ordinates u, v , which satisfy the equation.

In geometry it is the curve, whether defined by means of its equation or in any other manner, which is the subject for contemplation and study. But we also use the curve as a representation of its equation; that is, of the relation existing between two magnitudes, x, y , which are taken as the co-ordinates of a point on the curve. Such employment of a curve for all sorts of purposes—the fluctuations of the barometer, the Cambridge boat-races, or the funds—is familiar to most of you. It is in like manner convenient in analysis, for exhibiting the relations between any three magnitudes, x, y, z , to regard them as the co-ordinates of a point in space; and, on the like ground, we should at least wish to regard any four or more magnitudes as the co-ordinates of a point in space of a corresponding number of dimensions. Starting with the hypothesis of such a space, and of points therein, each determined by means of its co-ordinates, it is found possible to establish a system of n -dimensional geometry analogous in every respect to our two- and three-dimensional geometries, and to a very considerable extent serving to exhibit the relations of the variables.

It is to be borne in mind that the space, whatever its dimensionality may be, must always be regarded as an imaginary or complex space, such as the two- or three-dimensional space of ordinary geometry. The advantages of the representation would otherwise altogether fail to be obtained.

I omit some farther developments in regard to geometry, and all that I have written as to the connection of mathematics with the notion of time.

(To be continued.)

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

STATE INSTITUTIONS.

Illinois state laboratory of natural history, Normal, Ill.

Experiments with diseased caterpillars.—Prof. S. A. Forbes is making a special study of 'schlaffsucht,' or some very similar disease, among our native caterpillars. He has so far proven that the disease is characterized by an enormous development of bacteria in the alimentary canal, the same forms appearing in the blood before death; that it is contagious by way of the food ingested; that the characteristic bacteria may be easily and rapidly cultivated in sterilized beef-broth; and that caterpillars whose food has been moistened with this infected broth, speedily show the bacteria in the alimentary canal, and, later, in the blood, and soon all die of the disease. Other caterpillars of the same lot, receiving the same treatment, except that the food is moistened with distilled water instead of the infected broth, remain unaf-

fected. These bacteria are likewise cultivable in vegetable infusions, but multiply there less freely.

Every step of the investigation is fortified by stained and mounted preparations, which are being submitted to cryptogamists. It has already been determined that the bacterium infesting a brood of *Datana ministra* in his breeding-cages is identical with the *Micrococcus bombycis* of the silk-worm; the form, measurements, modes of aggregation, and behavior to reagents, of the two, being the same. *Datana Angusii*, feeding upon walnut, was also occasionally infested by this *M. bombycis*, but much more commonly by a spherical species, probably undescribed.

In the cabbage-worm (*Pieris rapae*) occurs still another species of *Micrococcus*, very minute (5μ in diameter), globular, and usually either single or in pairs. This is far the most virulent of the insect affections, which is being studied by Forbes,—the

most like a plague. In its earlier stages it can usually be recognized by the light tint of the larvae, an ashy green, so different from the ordinary color that one may pick out the diseased worms at a glance. These soon become torpid, and commonly die in a few hours. After death, decomposition is peculiarly sudden and rapid. A pale individual, picked out in the evening while still active, at eight o'clock the following morning was dead, blackened, and almost deliquescent, the whole body being reduced to a semi-fluid condition. This *Micrococcus* multiplies rapidly in beef-broth, rendering the fluid turbid.

The cultures of these *Micrococci* are made by the most rigorous use of the modern methods of 'pure culture.'

Only *M. bombycis* has thus far been successfully used by Forbes for the infection of healthy larvae; but experiments with the other species are now in progress. Measures are also being taken to learn the length of life of these bacteria when kept in hermetically-sealed tubes, with the expectation that this will furnish a means of preserving and transporting them for practical use, if this should prove to be worth while.

Forbes is also experimenting with the various ferment-germs appearing spontaneously in organic infusions, and has noted the occasional appearance of large numbers of *Saccharomyces* in the intestines of unhealthy larvae, and of those whose food has been treated with fermenting vegetable infusions.

NOTES AND NEWS.

We deeply regret to announce the death of Dr. Hermann Müller, on Aug. 25. Next to Darwin, Müller has done the most to advance our knowledge of the mutual relations between plants and animals in one of its many phases. Some notice of his life and work will be given in a future number.

—The boundary-line between Guatemala and Mexico, which, as we announced last week, Mr. Miles Rock has been commissioned to locate, is about two hundred miles in length; and one or two years will be required to finish the work. Astronomical stations will be established along the line, and topographical and profile maps will be made to extend as far as time and means will permit. If possible, the longitude of Guatemala City will be determined telegraphically by connecting with some point on the coast occupied by the U. S. hydrographic party under Lieut.-Commander Davis.

Mr. Rock has also been commissioned by the Smithsonian institution to collect notes on anthropology in the country over which his survey extends, and to photograph whatever archeological ruins he may meet with during the progress of the survey. He sailed from New York on Oct. 1, in the steamer *Acapulco*.

—The annual report of the librarian of the public library of Cincinnati for the year ending June, 1883, has just been issued. The total number of volumes and pamphlets in the library is 149,750. "The average number of books loaned daily for home use

has been 680. The average number delivered for use in the reading-room has been 379 per day." In tables showing the number of books issued for home use and for consultation are given percentages for various classes. Fiction heads the list with 81.4% in books for home use, and 28.3% in the reading-room. Science and arts are represented by only 2.9% for home use, but rise to 24.8% for books consulted at the library. The number of volumes of fiction circulated during the year was 167,678, and of science and arts only 5,928. In the consulting-room, however, 39,539 volumes of fiction were issued, and 33,916 volumes in science and arts. Though these figures show a marked preponderance in the circulation of fiction over science and arts, as indeed they do over every other class, the preponderance is perhaps more apparent than real. As the librarian says in his report, these percentages are often misleading. "They lead the public to believe that a much larger than a true proportion of the work of a library is in the distribution of books calculated to entertain rather than to instruct. Probably not more than one-sixth of the time devoted to a volume of history or of science is devoted to a novel by the average reader; and yet in these figures volumes of history and science count equally with volumes of fiction and juvenile literature."

In a table 'showing the number and the classes of books used during each month of the year,' we find some interesting figures. More books were used during the months of January and March than during any other two months of the year. In philology there was nearly a regular increase from month to month from July to January, and a decrease to June. In history, from 1,387 volumes in December, there was an increase to 1,818 in January, decrease to 1,385 in February, and increase again to 1,586 and 1,581 in March and April respectively. In geography and travels, March takes the lead with 1,006. In science and arts the increase is regular from July (2,558) to January (4,656), when the decrease commences; and in June we have 2,838. In the totals we find that nearly 40% of the books were used during the months of December, January, February, and March; while only about 28% were used during June, July, August, and September.

—The latest news from the French deep-sea explorations on the *Talisman* is comprised in a letter from M. Alph. Milne-Edwards, at Teneriffe. Every thing had worked in a satisfactory manner. Many soundings had been made off the coast of Morocco, and interesting profiles of the bottom thereby developed. Bottom and water specimens were simultaneously obtained, and the work was even carried on at night by the aid of electric lights. Considerable zoological collections had been made, and the professor was especially devoting himself to the study of their distribution in depth. The character of the fauna already enabled a tolerable estimation of the depth to be made from an examination of the animals contained in any particular haul of the dredge. By the use of extremely large nets, better luck had been secured in the capture of deep-sea fishes than had

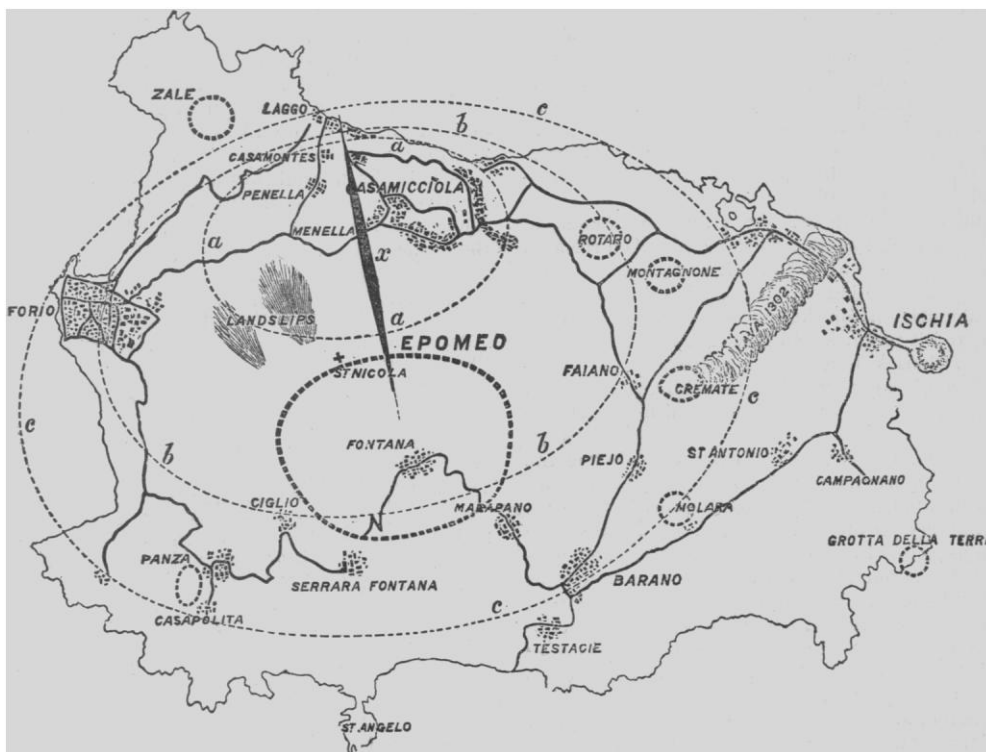
previously attended their efforts, and a large number of specimens had been obtained. On leaving the Canaries, the expedition would proceed to the Cape Verde Islands, and thence along the so little known African coast.

—Mr. H. J. Johnston-Levis publishes the accompanying map of Ischia (scale 1 : 80,000) in *Nature*, with some further account of the recent earthquake. In company with Prof. P. Franco of Naples, he travelled over the whole island without detecting any sign of volcanic action. If isoseismal lines are drawn over the injured districts, we find, he says, "that they assume the form of elongated ellipsoids, whose major axes run nearly east and west." In the

surface. The rupturing of this plate-like fissure was apparently greatest at a point nearly midway between its extremities." The ancient eruptive centres and craters are marked on the map in dotted circles.

—Ensign S. J. Brown, U. S. navy, has been elected professor of mathematics in the navy, and assigned to duty at the Naval observatory in Washington.

—Thouar writes from La Paz, under date of May 31 last, that in his search for Crevaux he had arrived by the way of Tacna, across the Cordilleras, on the 28th. The Bolivian government, by Sig. A. Quijarro, minister of foreign affairs, had shown great interest in the plans, and desire to assist to the extent of its power. An expeditionary corps had been



first isoseismal area, *a, a*, destruction was total; in the second, *b, b*, many houses are fallen, and the rest will require rebuilding; in the third, *c, c*, they were severely fissured; the fourth, in which houses were only very slightly fissured, not only includes the whole island, but must extend into the sea some distance. "From a careful examination of observed azimuths and angles of emergence, all point to a plate-shaped focus, whose strike extends in a line from Fontana, just west of Menella, to near the beach at Lacco. The plane of this fissure is probably roughly perpendicular to the surface, but may slightly dip towards the east, as the isoseismals are slightly nearer on the eastern side of the seismic vertical, which, as a necessity, is not represented by a point, but a line on the

equipped, and directed to march on Teyo, the Toba capital, which it is designed to occupy while part of the force descends the right bank of the Pilcomayo to Ascension. This expedition should have left Caiza in the month of June last, while, on the 3d, Thouar intended to start for Caripari, *viâ* Oruro, Potosi, Sucre, and Tarija.

—Endeavors have been made during the past session of the English parliament to obtain such amendments of the Factory acts as would protect not only the overworked and overheated workers in the bake-houses, but those desperate men who face certain death by poisoning in the manufacture of white lead. No act of parliament, however, will be of any real use until some improved process makes safety as

cheap as danger. Lately, with this object in view, Prof. C. Gardner has perfected an invention through which, by electrical energy and the cheap production of carbonic acid, applied through a special apparatus, in combination with the necessary acid vapor at the proper temperature, the formation of white lead of the purest color and best quality is rapidly and cheaply carried out in closed chambers; the lead resting upon shelves, which are lifted out when converted, and emptied, without any dust being raised, into a combination of machinery closed in, from which it comes forth as white paint ground ready for the market, or, if required, as dry powder. In either case the dangerous operations of the 'white bed,' and washing and stoving, are completely done away with, and no opportunity is given for the dust to enter the air, or touch the persons of the workers.

—Mr. G. Brown Goode, U. S. commissioner to the fisheries exhibition, sailed from London for the United States on the 19th ult.

—The summer station of the U. S. fish-commission at Wood's Holl, Mass., will remain open until about the 20th of this month, at which time the commissioner will return to Washington.

—Dr. R. W. Shufeldt, U.S.A., who was engaged in making collections in Louisiana, has been released from duty on account of ill health.

—Mr. Robert Ridgway has left his duties at the national museum for the present on account of ill health, and is recruiting in New York.

—Dr. Charles Rau has in preparation a monographic work upon prehistoric fishing-implements. It will be published as one of the Smithsonian Contributions to knowledge.

—Professor Lester F. Ward has returned from the west. He reports having thoroughly explored about seventeen hundred miles of the Missouri River.

—The very prevalent idea that aniline dyes have poisonous properties has inspired the German chemist, Dr. Grandhomme, to investigate the subject as illustrated in the coal-tar color-works of Messrs. Lucius and Brüning at Höchst-on-the-Main. Nowhere else could these researches have been conducted in so satisfactory a manner, as the Höchst color-works employ six hundred and seventy-two men in the actual manufacture of the colors, exclusive of their large staff of mechanics and laboratory assistants. One regulation provides that no workman shall enter any other department than his own; so that the works offer an excellent field for accurate observation. The following results were obtained by Dr. Grandhomme from personal observation, and the tables of accident and illness kept in the works. Nitrobenzol is known to be poisonous, yet symptoms of nitrobenzol-poisoning only appeared in the cases of illness reported in that department during four years. Aniline is unquestionably poisonous; yet, out of one hundred and seventy-one cases of illness in that department, only eighteen were due to aniline: none were fatal, and the average duration of the illness was one and one-half days. Magentas made by the arsenic process, and duly purified, are recognized as poisons; but the symptoms recorded are

those produced by arsenic, which, in some inferior magentas, exists in the proportion of even eight per cent. No illness caused by pure magenta was recorded at Höchst, and aniline no more exists in the finished magenta than manure exists in wheat. In the department where blue colors were made, only one case of aniline-poisoning was recorded; none in the violet and green departments. A special disease appeared in the eosine department, causing extreme perspirations from the pores of the hands, but not among the men employed in packing the finished colors. No special disease was noted in the naphthol and alizarine departments. The use of alcohol was found to reduce the power of the constitution to bear the action of aniline: so no alcoholic drinks were allowed in the Höchst works, and no men addicted to drinking admitted.

—In Namaqua-land, South Africa, no rain has fallen since Aug. 15, 1881, and plants, animals, and men are dying of drought and starvation. Wheat and seeds have been sent by the Cape Colony, and a relief committee has been formed.

—Tillo has determined the total length of navigable rivers in European Russia, which is only 72,000 kilometres for that vast territory, a deficiency due to the dryness of the climate.

RECENT BOOKS AND PAMPHLETS.

Bacharach, M. Abriss der geschichte der potentialtheorie. Göttingen, *Vandenbroeck & Ruprecht*, 1883. 3+78 p. 8°.

Birnbaum, K. Die prüfung der nahrungsmittel und gebrauchsgegenstände im grossherzogthum Baden und die resultate einiger in der mit dem chemischen laboratorium des polytechnikums in Karlsruhe verbundenen prüfungs station ausgeführten untersuchungen. Karlsruhe, *Braun*, 1883. 8+119 p., 1 pl. 8°.

Brass, A. Biologische studien. theil i.: Die organisation der thierischen zelle. heft i. Halle, *Strien*, 1883. 8+80 p., 4 pl. 8°.

Claus, C. Fragment einer monographie des platins und der platinmetalle, 1865-83. Leipzig, *Voss*, 1883. 5+92 p. 8°.

Handwörterbuch der chemie. Herausgegeben von Prof. Dr. Ladenburg unter mitwirkung von Dr. Berend, Dr. Biedermann, Prof. Dr. Drechsel, etc. band i. Breslau, *Trewendt*, 1883. 8+712 p., illustr. 8°.

Hopkins, Louisa Parsons. Handbook of the earth: natural methods in geography. Boston, *Lee & Shepard*, 1883. 78 p. 24°.

Kaempfer, D. Ueber die messung electrischer kräfte mittelst des electrischen flugrads. (Inaug. diss.) Berlin, *Friedländer*, 1883. 36 p. 8°.

Löwe, O. Ueber die regulären und Poinso'schen körper und ihre inhalts bestimmung vermittelst determinanten. München, *Rieger*, 1883. 28 p., 1 pl. 8°.

Merkmal, das verlorene, des winkel-begriffes eine folge der fortschreitenden bewegung auf dem gebiete der geometrischen formenlehre nach wesentlichen ideen und neuen gesichtspunkten. Teschen, *Cotula*, 1883. 23 p. 8°.

Petzoldt, K. Petrographische studien an basaltgesteinen der Rhön. (Inaug. diss.) Halle, *Tausch*, 1883. 48 p. 8°.

Samuels, E. A. Our northern and eastern birds. New York, *Worthington*, 1883. 600 p., illustr. 8°.

Schmitz, F. Die vegetation des meeres. Bonn, *Strauss*, 1883. 21 p. 8°.

Smoke abatement committee, 1882, report of. With reports of the jurors of the exhibition at South Kensington, and of the testing engineer, to which are added the official reports on the Manchester exhibition. London, *Smith, Elder, & Co.*, 1883. 14+193 p., 76 pl. 4°.

Taugermann, J. D. Licht, harmonie und kraft. Eine naturwissenschaftlich-philosophische studie. Leipzig, *Mulze*, 1883. 70 p. 8°.

Tischner, A. The sun changes its position in space, therefore it cannot be regarded as being 'in a condition of rest,' Leipzig, *Fork*, 1883. 37 p. 12°.